

Historical Collection

SERIAL

Columbia University
in the City of New York
College of Physicians and Surgeons
Library



AUG 11 1944

D15

THE
PHILADELPHIA JOURNAL
OF THE
MEDICAL AND PHYSICAL SCIENCES.

SUPPORTED BY AN ASSOCIATION OF PHYSICIANS,

AND

EDITED BY N. CHAPMAN, M. D.

PROFESSOR OF THE INSTITUTES AND PRACTICE OF PHYSIC AND CLINICAL
PRACTICE, IN THE UNIVERSITY OF PENNSYLVANIA.

"In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*"

Edinburgh Review, No. I.XV.

VOL. II.

PHILADELPHIA:

M. CAREY & SONS—CHESNUT STREET

William Fry, Printer.

1821.

Eastern District of Pennsylvania, to wit:



BE IT REMEMBERED, that on the fourth day of May, in the forty-fifth year of the Independence of the United States of America, A. D. 1821, M. Carey & Sons. of the said District, have deposited in this office the title of a Book, the right whereof they claim as proprietors, in the words following, to wit:

“The Philadelphia Journal of the Medical and Physical Sciences. Supported by an Association of Physicians, and edited by N. Chapman, M. D. Professor of the Institutes and Practice of Physic and Clinical Practice, in the University of Pennsylvania.

“In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*”

Edinburgh Review, No. LXV.

VOL. II.”

In conformity to the Act of the Congress of the United States, entitled, “An Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies, during the times therein mentioned”—And also to the Act, entitled, ‘an Act supplementary to to an Act, entitled, ‘an Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies during the times therein mentioned,’ and extending the benefits thereof to the Arts of designing, engraving and etching historical and other Prints.”

D. CALDWELL,

Clerk of the Eastern District of Pennsylvania.

CONTENTS.

- I. An Account of the Yellow or Malignant Fever, which appeared in the City of Philadelphia, in the Summer and Autumn of 1820, with some observations on that disease. Read before the Academy of Medicine. By Samuel Jackson, M. D. President of the Board of Health. *Continued from No. 2,* 5
- II. On the Medical Character of the United States. By S. Colhoun, M. D. 39
- III. Cursory Remarks on a disease vulgarly called Milk Sick. By William W. Lea, M. D. 50
- IV. A Description of the Anatomy of the Urinary Bladder, with some of its appendages, as concerned in the operation of Lithotomy. By William Anderson, Esq. Licentiate of the Royal College of Surgeons, Edinburgh. In a letter to Professor Chapman, M. D. &c. &c. 55
- V. Observations on the Use of Prussic Acid in Phthisis Pulmonalis. Read before the Academy of Medicine of Philadelphia. By Joseph G. Nancrede, M. D. 66
- VI. Strictures upon Dr. Merriman's Opinions of Retroversion of the Uterus and Extra-Uterine Conception. By William P. Dewees, M. D. 76
- VII. The History and Treatment of Bony Tumours. By William Gibson, M. D. 121

CASES.

- VIII. An Account of a *Lusus Naturæ*. By John Livingstone, Surgeon of the British Factory at Canton. Communicated by J. R. Mitchell, M. D. &c. 148
- IX. Cases illustrative of the Use of Mercurial Ointment in Erysipelas, Swelled Leg, &c. &c. Communicated by P. W. Little, M. D. 153
- X. A Case of Hydrocephalus, communicated by Joseph Glover, M. D. Surgeon General of the State of South Carolina, 159
- XI. A Case of Gangrenous Suppuration. Read before the Academy of Medicine of Philadelphia, and communicated by John Barnes, M. D. one of the Vice Presidents of the Philadelphia Medical Society, &c. 168

- XII. A Case of Carbuncle, with some remarks on the Use of Caustic, in that disease. By Philip S. Physick, M. D. . 172

REVIEW.

- XIII. The History of the Practice of Vaccination. By James Moore, Director of the National Vaccine Establishment, Surgeon of the Second Regiment of Life Guards, and Member of the Royal College of Surgeons in London. . . . 176
Medical and Philosophical Intelligence, ... 197

TO READERS AND CORRESPONDENTS.

WE acknowledge, with many thanks, the receipt of communications from Drs. Beck, Dickson, Atlee, Bell, Wilson, La Roche, Coates, Meigs, and Edward Barton, which shall appear in the next number of the Journal.

Doubts having recently been expressed of the validity of Dr. Physick's claim to the original suggestion of the *animal* ligature, we have collected evidence which puts this point beyond the possibility of cavil or controversy.

That the first practical application of the ligature was made by him is admitted. But it is alleged against his title, that the suggestion of the ligature occurs in Young's "Medical Literature," contained in the following sentence: "I have often wished to try ligatures of catgut, which might be absorbed." The work of Dr. Young was published in *eighteen hundred and thirteen*, and we have the declaration of Dr. Gartley of Norristown, that while a pupil of Dr. Physick, between the years *seventeen hundred and ninety-eight* and *eighteen hundred and one*, he heard him propose such a ligature as superior to those then in use, and that he well remembers his fellow-student, the late Dr. Seip, who was present at the conversation, suggested for this purpose, slits of the tendons of the deer, as having great toughness and strength.

This statement is confirmed by Dr. Vandyke of Abington, who says, that at a surgical operation some years ago, he heard Dr. Gartley mention the same facts.

THE
PHILADELPHIA JOURNAL
OF THE
MEDICAL AND PHYSICAL SCIENCES.

ART. I. *An Account of the Yellow or Malignant Fever, which appeared in the City of Philadelphia, in the Summer and Autumn of 1820, with some Observations on that disease. Read before the Academy of Medicine.* By SAMUEL JACKSON, M. D. President of the Board of Health.

(Concluded from No. 2.)

Treatment of the Disease.

IT is a subject of well founded complaint, that it has been a prevailing vice with medical writers, to indulge in partial, encomiastic, and, it is to be feared, sometimes fanciful representations of the powers and efficacy of some favourite remedy or mode of practice, to the introduction of which into medicine, they have been instrumental, but which have not been justified by subsequent experience. Sanguine expectations have been in this manner too often injudiciously excited, that have terminated in disappointment dark and bitter, as the hopes they had nurtured were bright and pleasing. How many of the articles of the former materia medica, celebrated for their virtues, are now known to be inert and useless! How many of our present medicines have been invested with curative energies for formidable diseases, in which a sober and matured experience has proved them to be of none or of little avail! What different systems

and modes of treatment, founded on baseless theories, partial views, and limited observation, have enjoyed their short-lived reigns, extolled and defended by misjudging partizans, but which now lie entombed in dusty and undisturbed tomes, and are brought into recollection only to display the errors they embraced, and the follies of their pretensions!

With writers on epidemics, the neglect of a proper discrimination of the different grades and classes always found to exist in those diseases, and a statement of the result of the treatment in each of them, is a common cause of error and deception, that tends to confuse, and lead the inexperienced and unwary into misconceptions and mistakes. Inferences founded on observations confined to one class alone, must be deceptive, and wholly inapplicable to the others, while the prognosis established on so limited a view, cannot prove otherwise than fallacious. It is from this want of precision and discrimination, that various and inconsistent histories of the same epidemic disease, are given by different writers; and that courses of treatment the most opposite are equally commended, which, when its recurrence brings the disease within the sphere of our own observation, are equally contradicted by the course of our experience.

The wide difference that existed between the character and treatment of the disease last summer and autumn, and the descriptions contained in some of the most popular and esteemed works that treat of it, has given rise to the preceding reflections. The tone of confidence in which Dr. Rush speaks of the medicable nature of yellow fever; of its easy and speedy subjugation by the aid of rapid, early and powerful depletion; the certainty, that Chisholm ascribes to the mercurial treatment; and the late testimony of Dr. James Johnson to the curative wonders of the lancet and calomel, are calculated to inspire the most perfect reliance on their efficacy. Such was the impression which had been conveyed to my mind, that I felt a strong assurance, that should my path be crossed by this most formidable of diseases, it would only be to witness the most successful

efforts, and the most splendid triumphs of the science of medicine. Never was disappointment more complete, or professional pride more humbled.

The disease, when it first appeared at Hodge's wharf, was encountered with depletion, active purging, calomel freely and largely administered, with a view to salivate, and blisters; but one after another, its victims were carried off, without an impression having been made, or its fatal tendency alleviated. The pulse in no instance was tense, or refused to yield to venesection and cathartics, but was speedily reduced by them, both in frequency and force. In several, it became quite natural, and the skin cool, yet with the disappearance of the common diagnostics of fever, there was no favourable change in the general symptoms, which continued to evince an unabated malignancy; while the disease hurried on so rapidly to its termination, that it was in vain to calculate on the specific effects of mercury. Two only recovered in this quarter, who exhibited the well-defined symptoms of the disease. In one, attended by Dr. E. Barton, the mouth became sore on the third day, continued stationary for two or three days, during which, the patient was in great jeopardy, then became more affected, and the disease gave way. The other was attended by Dr. Knight, and was trusted chiefly to cathartics, which produced a speedy mitigation of the symptoms.

In the case of Mr. Hill's servant girl, the mercurial treatment was pushed with vigour to a great extent. It was commenced within four hours from the attack. Thirty ounces of blood were previously drawn, the pulse being one hundred and thirty strokes per minute, and full. Five grains were administered every three hours. Senna and salts, and calomel and jalap were interposed occasionally, as purgatives. The bowels were obstinately constipated throughout her illness. On the second day, a scruple of calomel was given at a dose, and calomel gr. viii. pulv. antimon. gr. iii. opium gr. i. were exhibited every three hours. Another scruple was given on the third day, and the same powders continued. Mercurial frictions were employed, and the

blisters were dressed with mercurial ointment. The case terminated in death on the fifth day. Near four drachms of calomel were administered, but not the slightest tenderness of the gums was produced.

Mrs. Philly, who was attended by Dr. Clark, was treated in a manner nearly similar, though with less vigour, but with the same unfortunate result. She was bled twenty-five ounces, and purged with calomel, which was also exhibited with the intention to salivate. The disease in this case was insidious, presenting no very alarming symptoms, until the evening preceding dissolution.

At the same time, the disease was witnessed in Mrs. Double, a tenant of the house in which Mrs. Philly resided, abandoned nearly to its course, uncontrolled by any remedial means. The confusion attendant on the hasty removal of the inhabitants, and procuring accommodations out of the city, for such as could not provide a refuge for themselves, and the want of a regular attendant, caused her to be neglected. Yet the symptoms did not assume a more malignant aspect, than occurred in many of those, who were under regular and careful treatment. In fact, in the cases of the first class, the disease moved steadily on, from stadium to stadium, unchecked, and indeed unaffected by any treatment pursued, nearly the same as if left to contend with the mere efforts of the constitution.

Two cases of a light character were treated with cathartics and calomel. The evacuations from the medicine were copious, and produced an immediate alleviation of the symptoms. On the third day the mouth became affected, and convalescence was at once established. The treatment in thirteen of the cases that occurred in this locality of the disease, was similar, consisting of depletion by drawing blood, active purging, and calomel in small and repeated doses, intended to salivate. One case of the second class, and of a high grade, in which the mouth became sore, recovered, and three cases of the third class, in two of which the system was under the mercurial action, also were restored to health.

The disease at Walnut Street, and derived from that source, was more diversified in its nature, than at Hodge's wharf. The cases of the first class were less numerous, the disease consequently more manageable, and success more frequently crowned the treatment pursued.

Different modes of practice were adopted, but I cannot say, that the result authorises a decided preference in favour of any one of them.

Early depletion with the lancet and cathartics, were relied on by some; some trusted to cathartics, sudorifics, calomel in small and repeated doses, and mercurial frictions; others combined both the preceding plans; while at the City Hospital, under the medical charge of Doctors Hewson and Chapman, counter-irritation excited by ol. terebinth. became a leading and favourite remedy. In the cases of the first class, these different modes of treatment seemed to be of equally little avail, and in the other classes, each may boast nearly an equal number of recoveries.

With Mr. Annesly, who was one of the first affected in this quarter, the disease commenced with symptoms of great violence. He was attended by Dr. Rush, who bled him copiously. An immediate abatement of the disease ensued, and he rapidly recovered. Mr. Smith and Mr. Barker were both depleted freely, and the mercurial course pursued with them; the purging and mercurial plan was adopted in the case of Mr. Edwards; but in each, the termination was fatal. In a case that occurred at this time, blood was drawn twice the same morning, and cups applied to the head. The blood on the second bleeding was covered with a soft, jelly-like coat of buff, of a greenish hue. Huxham mentions having observed this appearance of the blood in putrid malignant fevers, "the superior part being a livid gore, or a kind of dark green, and exceedingly soft jelly. The pulse in these cases," he observes, "sinks oftentimes surprisingly after a second bleeding."* This effect was produced in the case alluded to. The pulse in the evening was a mere thread, and the patient expired the next morning.

The disease manifested itself with considerable violence in Mrs. Duffy. Thirty ounces of blood were abstracted a few hours after its development, and were followed with a strong dose of calomel and jalap. In two hours an infusion of senna and salts was given, and calomel gr. viii. and pulv. antimon. gr. iv. were ordered every three hours. The alvine evacuations were copious, and produced an immediate abatement of the untoward symptoms. A very free diaphoresis took place on the second day; and on the third day she was removed into the country.

Miss Jane Mann refused to be bled at the time the disease first invaded her system, but took a large dose of calomel and jalap, and powders of calomel and pulv. antimon. were given every three hours. The evacuations from the bowels did not diminish the intensity of the symptoms, which, on the contrary, became more aggravated. At the end of the second day Dr. Currie saw her with me. The skin at this time was hot and dry, the face highly flushed, the pulse one hundred and twenty, full but not tense, the carotids beat with great violence, the eyes were red and watery, cephalgia acute and distressing. The indications for bleeding appeared so obvious, that fifteen ounces of blood were taken away. The mercurial treatment was continued with the addition of frictions, and occasional purgatives to evacuate the bowels. In four hours after the bleeding, an evident change was observable. The skin had become cool, but was harsh and dry, and the pulse was natural as to frequency and force. The malignancy of the disease was not, however, subdued, or the danger of our patient lessened. They had on the contrary evidently increased. She was now slightly comatose, her eyes were more deeply injected, the evacuations passed involuntarily, and the yellow hue was appearing about the neck. These unfavourable indications continued to increase. Blisters to the legs, arms, and epigastric region, scarcely affected the skin, evincing its declining sensibility. She expired on the fifth day, in convulsions. The mercury had produced no effect on the mouth.

A tolerably free ptyalism was raised in young Room, under the care of Drs. J. Nancrede and Monges, which lasted for twenty-four hours. The disease then obtained the ascendancy, the salivation disappeared, hæmorrhage from the lips and black vomit ensued, which the next day terminated in death.

In the Wharton family, in which the disease proved so fatal, the sudorific and mercurial treatment were employed and chiefly confided in. In the case of the daughter, its efficacy in the violent grades of the disease was fairly tested. It was brought into operation a few hours only after the commencement of the disease, and was steadily and perseveringly adhered to, under the faithful and devoted attention of Drs. Parrish and Wood. Extensive frictions with the ointment were employed, and calomel was administered in small and repeated doses. On the fourth day the gums became sore, and a slight ptyalism was induced, which, however, soon subsided before the increasing force of the disease. Black vomit supervened the succeeding day, and she was numbered among the victims that had already fallen at the shrine of this fatal malady. The mercury produced no effect on either of the sons.

The mercurial treatment proved successful in the case of young Mr. King, and the black girl of that family, attended by Dr. S. P. Griffiths; and Miss Ann Sprowle recovered under the depleting and mercurial treatment, in the hands of Dr. Coats, though it proved ineffectual in the case of the mother.

Many other cases, which it is unnecessary to particularize, were treated in a similar manner to those already mentioned. The ill-success that attended what was considered the regular and established practice in malignant fever, gave rise to a desire to seek for some more powerful auxiliary, than was then at command. The stomach being considered the part of the system, in which the diseased action first commences, and from which it is extended by sympathetic or associated action to all the other portions, it was believed possible to strike at the disease in its strong

hold. The excitation of counter-action in affections arising from local irritations, is founded on the best established and most approved principles in medicine. Blisters are the best remedies for erysipetalous, and some species of phlegmonous inflammations; turpentine for the healing of burns; and cayenne pepper in some cases of angina. It was conceived, therefore, that an irritation excited in the stomach, might establish a new action, different from that produced by the poison, that occasions malignant fever. On this principle the free and liberal exhibition of the ol. terebinth. in conjunction with some other essential oil, was commenced at the City Hospital. The practice was not entirely novel. In 1805, it was tried in the City Hospital by Dr. Parrish. Turpentine was, in that year, administered in a number of cases, in considerable doses, and was also exhibited in injections. The beneficial effects were not so decisively pronounced, as to acquire for it a high degree of confidence.

The first case in which it was used the last year, was in that of L. Moore, one of the sailors of the Brig Martha. He had been previously sweated and purged. It was commenced on the fifth day of the disease. At that time, the pulse was quick and weak, the feet and hands cold, nails purplish, and a violent hiccup attended. Thirty drops of a mixture, consisting of ol. terebinth. ℥viii. ol. menth. ℥i. were given every hour, beginning at 5 o'clock P. M. In the morning the hiccup was unabated, the extremities were warmer, pulse fuller and slower, and expression of countenance improved. The dose was increased to a teaspoonful. In the evening the hiccup had ceased, and the patient became convalescent.

The second trial was with Mrs. Baird, who was also in the fifth day of the disease. Previously to being sent to the Hospital, I had purged her freely, and she had been on the mercurial treatment for two days. She was very low and the case considered desperate, when the turpentine was commenced. Her skin was yellow, cool and dry; the respiration slow, accompanied with deep and heavy sighing; pulse soft and slow; frequent retching and vomiting of dark

coloured fluid. It was thought, she would not have lived through the night. A tea-spoonful of the mixture was given regularly every hour. An improvement took place under its use. The retching and vomiting abated; the pulse became firmer; the skin felt more natural. Her existence appeared to be prolonged by the medicine, but on the third day of its employment she expired.

The husband of the unhappy woman, the subject of the preceding case, was the next to whom the turpentine was exhibited. When his wife was sent to the Hospital, he entered into the service of the Board of Health, as one of the guards of the infected district. The first night of his watch, he sought an oblivion to his distresses in intoxication. In the morning he was found with the disease raging on him. He was sent to the Hospital; was immediately purged, and his stomach being highly irritable, a blister was applied to the epigastric region. The next day, the turpentine mixture was exhibited in the dose of a tea-spoonful every hour. The disease in this case was of a most ferocious character. The complexion on the second day, became mottled, from the stagnation of the blood in the capillaries; the pulse was nearly natural, the eye covered with injected blood-vessels; the abdomen extremely tender, gastric irritation distressing, and great anxiety and restlessness prevailed. Death put a term to these sufferings on the night of the third day.

The third case in which turpentine was administered, was the daughter of the unfortunate couple, whose melancholy fate has just been related. She had been sent into the west wing of the City Hospital, to which the poor inhabitants of the infected districts were conveyed, in order to remove them from the seat of pestilential infection. The day after her removal, I found her labouring under the disease. She was immediately transferred to the sick department. A mercurial purge was exhibited, and divided doses of calomel and pulv. antimon. were ordered. The next day, the turpentine mixture was substituted for the mercurial powders. In this case febrile action was open and manifest. The pulse was quick and rather full, the skin hot and dry,

constant delirium attended, and the stomach was irritable. Cold applications were directed to the head. The third day there was hæmorrhage from the nose, and a mercurial purge was given; delirium still continued. On the fourth day the unfavourable symptoms had subsided. The pulse was improved, the skin soft and moist, and delirium had ceased. The next day she was convalescent. The turpentine had been regularly given, from the time its use was commenced.

In Eliza Curtis, one of the young women sick in Letitia Court, the happiest effects were obvious from its employment. She was in the fourth day of the disease, when her case was made known. The pulse was very easily compressible, skin cool and dry, of a yellow hue round the neck and breast, stomach very irritable, strength greatly prostrated, fainting being induced when attempts were made to sit up or rise from bed. Sinapisms were directed to the lower extremities, and a tea-spoonful of the following mixture was given to her every hour. Turpentine, aqua ammon. āā ℥ii . ol. cinnamon ℥i . The medicine proved highly grateful to the feelings of the patient, who repeatedly asked for it. It was first given at eleven o'clock A. M. At six P. M. she was much better; the pulse was fuller and firmer; the stomach quiet; the tendency to delirium gone. The family having been removed, the Court completely emptied of its inhabitants, and about to be closed up, she was sent to the Hospital, and bore the ride well. She had subsequently hæmorrhage from the gums, but recovered under the use of the turpentine mixture, carbon. ammoniæ, and warm toddy, which were substituted for the turpentine towards the close of her disease.

Turpentine was also successfully employed by Dr. Harts-horne in the case of Mr. Evans.

It failed to produce any beneficial effect, administered to Dr. Worthington, and was also equally devoid of success in the case of Mr. Makinn, attended by Dr. Hewson.

In the Hospital, there were thirteen cases of the fever, in which the exhibition of turpentine was early commenced.

and in which a fair trial of the practice was given. Of those cases, eight recovered and five died.

The acetat: plumbi recommended in warm terms by Dr. Irvine, of Charleston, was brought forward after the repeated failures of the usual modes of treatment.

Dr. Worthington gave it to Miss Drinkwater in the last stage of the disease, and thought some advantage was gained by it. The vomiting was allayed, and the disease protracted, but it still proved fatal. One person recovered under its use in the hands of Dr. Wood; and Dr. Parrish thought it proved serviceable to young Scattergood, who recovered from a severe attack of the disease. Dr. Harvey Klapp also exhibited it to Davenport, one of the guards employed in the infected district. This case was the most protracted that occurred, but ended fatally with black vomit on the eighteenth day.

The disease in Duke Street, or Artillery Lane, and vicinity, was treated with purgatives and mercurials, and in one instance with small bleedings. Turpentine was tried by Dr. Cleaver in two cases. In one without any effect; in the other, Miss Bechtel, with evident disadvantage. She suffered severe pain in the chest and stomach after each dose, which soon compelled it to be discontinued.

In this quarter, where all died who were affected and remained in it, the disease was of that high degree of malignancy, that all modes of treatment were equally unavailing, and left a distressing conviction of the correctness of the observation, "*Imbecilior est medicina quam morbus.*"

From the foregoing general sketch of the different plans of treatment, that were adopted in the disease of the last summer and autumn, it is evident, that no one can claim, from its greater success, a pre-eminence over the others. The mercurial treatment certainly lost ground, and did not sustain the character that had been attributed to it. Many of the instances of its successful action, were cases of a light disease, that would have probably recovered under any kind of treatment properly directed. There were, however, three or four, in which its beneficial operation was

conspicuous. A struggle for mastery appeared to exist for a time between the disease and the remedy, while the fate of the afflicted patient hung in suspense. The establishment of a decided salivation became the harbinger of victory, and every untoward symptom disappeared. But in the far greater proportion of the more violent cases, and in those of the first class, its impression was not felt. It was opposing mounds of sand to the torrent in its fury.

An opinion that turpentine possessed great sanative powers, amounting almost to a specific action, and that a wonderful success, authorising such a notion, had attended its employment last year, was put into circulation and very generally entertained. The statement I have given of the precise result of that practice, will correct the erroneous impressions which were extensively made with respect to its efficacy. The practice at the Hospital was, I am of opinion, more frequently successful, than in the city, and turpentine was there the favourite remedy. This circumstance is not, however, to be attributed to any superiority in the practice or remedies employed, for it has uniformly occurred in every preceding epidemic. It is more correctly explained by the effects produced by a pure and salubrious atmosphere, constant medical attention, and good and faithful nursing. In several of the Hospital cases, moreover, the disease was of the milder grades, which would have recovered under any mode of treatment.

From the result of the experience acquired in our late disease, it is apparent, that we are not in possession of any remedies or system of treatment, that will enable us to cure yellow or malignant fever in its worst and most malignant form. In that class, the poison by which it is occasioned, produces organic lesions or structural derangement of vital organs to such an extent, that their restoration lies not within the compass of human power. Great is the confidence I possess in the powers of medicine, and exalted are the sentiments I entertain of the resources of the healing art. It is impossible not to be imbued with such sentiments and feelings, when we recal all that medicine has been able

to accomplish: the causes whence diseases spring made manifest, and thus disorders in themselves incurable, taught to be avoided, and plagues that laid waste the land, exterminated; the small pox, at first disarmed of its fury, and now almost blotted from our records; the scurvy, that scattered before it and broke the strength of fleets and armies, while yet untouched by the sword, rendered rare of occurrence, and harmless when it prevails; the monster Syphilis, bound in brazen chains,—“*vinctus ahenis*”—these and a thousand other benefits, that flow in never failing streams from this living source, command the admiration and gratitude of mankind, and beget an almost boundless confidence in the further and progressive improvements which medicine may yet achieve. But there is a limit placed, beyond which no human effort can extend, and where human skill and art cease to be availing. From the long experience, extended through many ages, of the constant mortality attending malignant and pestilential fevers; the uniformity with which they have baffled and defeated the wisest and most skilful of the sages of medicine; the millions that have been rapidly swept by them from the earth in short periods, “*ac cum cymba Charontis ad elysios campos citissime deferebantur*,” give us but too just cause to fear, they will remain the “*opprobria medicorum*,”—that for many succeeding ages, physicians must continue to use the language of Diemerbroek, and speak of them as of those “*inter quos mors sceptrum tenebat; plures enim moribantur quam evadebant: et inexplicabilis morbi malignitas omnium fere remediorum efficacium longe antecellere, eorumque vim respuere videbatur*.”*

Though we may feel ourselves thus hopeless and helpless in succouring the victims of those fatal diseases, when raging around us as epidemics, yet we can felicitate ourselves, that it is not our unhappy fate to be necessarily and inevitably subjected to their invasion. If we possess no antidotes to the fell poisons to which they owe their origin, we are enabled to prevent their production. Let us but

* De Peste, caput iii. p. 7.

wisely and prudently employ the means in our power, and correctly apply them, and it is not to be questioned, that we can ensure our safety against their ravages. But it is to be apprehended, that we too often make idols of our opinions, worshipping them with a blind and besotted devotion; and, like the heathen of old, compel all to join us in the cry, "Great is Diana of the Ephesians."

To acquire a knowledge of diseases and their causes, idle disputations, in which words are of more concern than facts, and the subtle reasoning of sophists more weighty than truth, will be of little utility. They are only to be known from oft-repeated and diligent observation, as Baglivi remarks, of the circumstances that are to be met with in every patient; from intelligence and acuteness of mind, fashioned to the method, and serving as the handmaids of nature. The history of diseases, he observes, is a peculiar and distinct science, which neither borrows its principles nor its improvements from any other, but which flow clearly and justly from the purest and brightest fountains of nature. They are to be studied beside the squalid beds of the diseased, in watching by night and in attendance by day; by frequent visits to hospitals and lazars; by noting with an untiring and intrepid patience—intrepida patientia—the frequent changes in the rise, the progression of the symptoms, and the effects produced by remedies; the benignity, the malignity, the violence, or mildness of the disease, as it appears at different periods.* These are some of the modes by which a knowledge of the causes and nature of diseases is to be acquired; and such is the discipline necessary to fit us for undertaking an investigation, surrounded with difficulties, that have confounded and eluded the researches of the wisest, for many ages. "*Hæc difficultas*," observes Diemerbroeck in his chapter "*de causis pestilentiaë, doctissimorum ingenia adeo torsit, et tam diversas opiniones, veritatis specie omnes exornatas, extorsit, ut ex eorum scriptis simul collatis, cum quo sentiendum sit, gravissime judicari queat.*"† If those whose education, habits of mind and observation,

* Baglivi de Praxi Medicina, caput v.

† De Peste, p. 15.

whose means and opportunities of obtaining the most correct information, and knowledge of facts; have been confessedly unable to arrive at any certain conclusions on the subject, how cautious ought those to be in pronouncing their judgments, who are destitute of those qualifying circumstances and advantages, and dependent chiefly on common rumour and vulgar reports,

Tam ficti pravique tenax, quam nuntia veri.

Æn. iv. 188.

for the materials on which to build their opinions.

Truth, like every thing of value to mankind, is difficult of production and slow of birth. It is never struck out at a heat, but is developed and perfected by passing through many and various operations, and being repeatedly submitted to the strictest scrutiny. On few subjects connected with science, have doctrines, taught in their inception, been found to stand the test of subsequent investigation. Is it then more likely that the sentiments respecting the nature of yellow fever, conceived in fear, and propagated in terror, when its destructive and cruel invasion was first announced, should be correct and free from error? While doubts on this point may with propriety be entertained, it is a solemn duty to continue the examination of this disease with minds unfettered with prejudices, and devoted to the cause of truth. While these doubts may justly exist, it would be the part of rashness, with a blind and partizan zeal, to enlist under either of the adverse parties, and venture our safety solely on the opinions of fallible men.

Appearances on Dissection.

Cadavera hominum morbis denatorum secunda sunt medico, manusque inquirande, ut inveniat, quæ morbi sit sedes, quæ causa.

BAGLIVI PRÆXEOS MEDICÆ, capt. v. p. 23.

Post mortem examinations took place in a large proportion of those, who died with the disease the last year. The observations made, confirmed generally those recorded by Dr. Physick in 1798 and 1799, Dr. Parrish in 1805, and other practitioners in this city, with the addition of a few particulars not instanced by them.

It will not be necessary to give the result of individual cases : but a general sketch of the phenomena exhibited by the different viscera, will be fully sufficient to point out the organs most obnoxious to the effects of the poison, and the seats of the disease.

The brain did not exhibit any marks of active inflammation. The veins of the dura and pia mater, were mostly very turgid with blood. Effusion of serum under the dura mater was found in three cases which had terminated with convulsions, and a larger proportion of it than ordinary appeared in the ventricles. The substance of the brain in no instance displayed any strong marks of disease.

The viscera of the thorax presented no appearances that indicated their partaking largely of the diseased action of the system.

It was amongst the abdominal viscera, that was to be discovered the evidence of the fatal storm, beneath whose fury the system had succumbed ; and of these, the stomach was a uniform and principal sufferer.

This viscus presented different appearances. I was much surprised to find it, on the first examination I made, without any marks of inflammation. The villous coat was of a rather whiter aspect than is usual, but a considerable quantity of black coffee-like fluid was contained in the stomach. In eight or ten instances, a nearly similar state of that organ was discovered, there being no inflammation, or a slight blush, mostly about the cardiac portion, being alone observable. The flowing out of the matter, that constitutes black vomit, appears to have relieved the loaded vessels in those cases, and to have terminated the inflammation ; but the death of the organ still ensued. It would seem, as I believe Dr. Physick has remarked in his dissections, that the formation of black vomit, is an effort of nature to terminate violent inflammation of the stomach. But in the far greater number of instances, the stomach was highly inflamed. The inflammation was always confined to the villous coat, the muscular and peritoneal escaping the affection. It was not uniformly diffused over the surface,

but would be deeper in one part than another. The cardiac portion was generally more inflamed than the pyloric, and sometimes a greater intensity was observable between the superior and posterior surfaces, a well defined and distinct line separating them. No erosions or abrasions were discovered, though the villous coat was at times nearly livid, and broke with ease, upon pressure with the nails. The vessels of the stomach were so turgid with blood, that portions of it cut out and dried, have formed very perfect preparations, exhibiting the ramifications of the vessels into their minutest divisions, in a very beautiful manner. The matter constituting black vomit, was met with in every examination. In two instances, in which it had been thrown up during life, with the usual characters, a fluid more resembling blood, was found after death.

The liver did not present any constant appearances, that could be considered as necessarily arising from the disease. It was sometimes gorged with blood, which flowed from it in a large stream, when an incision was made; and again appeared entirely destitute of it, not a drop following the scalpel, but the divided vessels being gaping and quite empty. But in these latter cases the veins of the omentum, mesentery, &c. were distended with blood. The bile in the gall-bladder was various in its qualities. In some, it was of the usual colour; in others of a reddish hue, as though tinged with blood; and in a number, it appeared of the consistence and colour of tar; but when spread on a white surface, was of a dark and beautiful green. It never bore the slightest resemblance to the fluid in the stomach. In two instances, the internal coat of the gall-bladder and ductus communis was inflamed.

The spleen and pancreas exhibited no unusual phenomena.

The intestines most commonly were more or less inflamed; not in a uniform manner, but in patches. They were in one subject contracted in some parts so much, that the little finger could scarcely be passed through them, and were swelled out and distended in other parts. Three or four

intro-susceptions were found in this case, but which were unattended with any inflammation at the spot where they existed. This patient had taken large doses of calomel, and had died strongly convulsed. I met the same circumstance in a subject, which had died with the common bilious fever. The intestines always contained considerable quantities of a black mucus, bearing a similarity to the flocculi of black vomit. In some cases it was evidently sanguineous.

The urinary bladder was sometimes very much contracted, and contained no urine. At other times small quantities were found in it. It was not inflamed, as it has sometimes been in yellow fever, in a single instance.

The veins of the omentum, mesentery, and, in fact, the whole system of the vena portæ, were always distended with fluid blood. It was at first supposed, that the blood being thus fluid, was in the dissolved state, so often mentioned by writers. But Dr. Hewson wishing to make some experiments, collected portions of it in cups. In the course of ten or fifteen minutes, it was firmly coagulated, and this was found in subsequent observations invariably to occur. The notion, therefore, of the blood being dissolved in this disease, frequently described by writers as observed in their dissections, is not correct.

The opinions that were held in respect to the nature of black vomit, were various and loose, until the examinations instituted by Dr. Physick, in 1798 and 99. It was, then, demonstrated very satisfactorily, that it proceeded solely from the stomach; that it did not partake in the slightest degree of the nature of bile, which had been the commonly received doctrine: and in fact, that the liver had no share in its production. Dr. Physick considers black vomit to be a diseased secretion from the vessels of the stomach. This opinion is entitled to great attention, and is rendered very probable by the arguments and experiments with which it is supported. But from the great turgescence of the whole portal system, always found distended with blood, I am disposed to believe that the inflammation of the stomach, and the other abdominal viscera, in this disease, is venous

and not arterial; arises from an engorgement of the veins, extending to their minutest division and first origin. Should this view be correct, black vomit, it is not unlikely, may arise from a sanguineous effusion from the capillary extremities of the veins. The matter of black vomit, does not maintain invariably the same characters, but recedes more or less from, or approaches to, an appearance of blood. I have seen several cases, in which the discharge, towards the termination, became nearly sanguineous, and a similar fluid was also found in the intestines.

Dr. Rhees, the resident physician at the City Hospital, instituted a series of observations on the black vomit, with a solar microscope. Innumerable quantities of animalculæ were found to exist in it. A single drop contained many thousands, being apparently a congeries of them. The black mucus of the intestines exhibited the same phenomena. When the matter fresh thrown from the stomach, was examined, the animalculæ were alive, and in constant motion; but if taken from the dead subject, or inspected after standing some time, they were always dead, and quiescent. Comparative examinations were made of the discharges from the stomachs of patients, ill with autumnal bilious and remittent fevers, but no similar appearances were discovered. These very curious observations require to be further and more extensively prosecuted, and diversified, in order to ascertain what relation those animalculæ possess with regard to the disease, either as cause or effect.

On Contagion.

The science of medicine, from the remotest ages, has been agitated with the contentions of its followers. Sect has conflicted with sect, and system been opposed to system, with a zeal more devoted to the ascendancy of party, than the triumph of the cause of truth. Baglivi lamenting the consequences of these discords, piously invokes the interference of heaven "*tantas componere lites,*" for the benefit of mankind and the good of the christian republic.—"*Has inter medicos pugnas et controversias rogo Deum.*"

Optim. Max. ut in magnum humani generis, et præsertim Christianæ Republicæ commodum componere velit, quo medicina tot retro sæculis miserè jactata, in placido tranquillitatis et concordiaë portu conquiescat.”*

Modern times, however, have not witnessed a more harmonious disposition, or any diminution of medical feuds and controversies. The torch of discord still burns, and probably in medicine no subject has been so fruitful of disputation, as the contagious or non-contagious nature of yellow fever—has excited more bitterness of feeling, or been handled with greater asperity. It is to be lamented, that this question, intimately connected with the most important interests of society, cannot be prosecuted in a spirit of true philosophy; and facts be investigated with an earnest desire to establish what is true, rather than to maintain preconceived opinions.

It appears at first view somewhat extraordinary, that so much diversity of opinion should exist, and continue this subject so long in suspense, when nothing more is required for its final determination, than an accurate observation and impartial verification of facts. But in truth, there is nothing more difficult or more rare, than correct observation. A thousand sources of error beset us, and, unconscious of their operation, we are deceived by them. Accidental coincidences are constantly occurring, having no relation, but which we connect as cause and effect, while our pre-judgments influence and often betray the operations of our senses. In wide spread epidemics, the limits to which the cause extends that occasions the disease, are unknown, and the poison being imperceptible, it is impossible except from its effects, to know where it may be conveyed and become accumulated. Hence inferences of its communication from individual to individual are liable to numerous deceptions, and can scarcely fail to prove fallacious. But when the disease prevails within certain bounds, and can in almost every instance be satisfactorily shown to have been contracted in them, by which the local source and ex-

* De Praxi Medica, p. 6.

istence of its cause are made manifest, our researches and conclusions are disembarassed from those equivocal circumstances, that perplex and confound them in epidemic periods. Such having been the state of the disease the last year, the most favourable opportunities were presented, of observing its capability of being communicated by the sick to those in health. Nearly one half of the cases of the disease, were scattered in different parts of the city, evidently contracted in some one of the original seats of the infection, that have previously been designated and described. Many of them were under circumstances in the highest degree calculated to aid its propagation, by means of contagion, did it exist. They occurred in the persons of the poor, in confined and ill-ventilated apartments, in houses crowded with inhabitants, in some of the filthiest and narrowest lanes, alleys and courts of the city, in which the negro epidemic had been or was still prevailing; yet, in conditions thus propitious to its propagation, not a single instance is known of any person attending on, or who had communication with the sick, or their apartments, having taken the disease. I will briefly point out a few of the most striking of those occurrences.

The family of Hays (in which occurred the first case that was reported to the Board of Health) occupied a single room of a house on the bank side of Water Street, midway between Race and Vine Street. It consisted of himself, wife and three children, who all shared the same *bed* during his illness. As there were no windows except in the front of the house, there could be no perflation through the room. The other tenants of the house, the neighbours, and his acquaintance, frequently visited him while sick, and a number of persons assembled at the house to attend his funeral. Not an individual thus exposed sickened, nor did any other case take place in that square, except Thompson's daughter, who contracted the disease of which she had a slight attack, by visiting at Mr. Hill's, near Hodge's dock.

A rigger of the name of Monier, contracted the disease at Walnut Street wharf. He lived in Mead Alley, where

he became sick and died. Two families occupied the house, the individuals of which had to pass through the room where Monier lay sick, in going in and out of the house. The neighbours also were frequently with him. The alley at that time, as it most generally is, was very filthy and offensive. No one took the disease from him.

A man of the name of Whitaker, also contracted the disease at Walnut Street wharf. His family resided in Kunkle's Court, and consisted of ten persons. The alley is narrow and confined, and generally filthy; the house small and close. No one of the family, however, or neighbours, were affected.

Thomas Thompson was a watchman on board of a ship that lay at Walnut Street wharf. He took sick at his residence in Callowhill Street, below Water Street, where he lived in a close, ill-ventilated apartment. None of the family, which was numerous, or of the vicinity, received any infection.

Elizabeth Mack, a German girl, took the disease on the wharf. She resided with her family in a small house in Strawberry Alley. She sickened and died there. None of the family, or those who had communication with her, became affected.

John Pounder, a pedlar, had the disease, and died in Small Street, a narrow and very foul lane. No one residing in it, or who was exposed by attendance on him, contracted the disease.

I attended a lad, who lay in a garret with a single window only in it. The disease displayed the worst features of malignancy. Seven or eight persons were almost constantly employed in the room with him, and the night previous to his decease, some one of them was incessantly occupied in frictions on his body. No sickness appeared among them.

A man who lived in Pegg's Street, near Pegg's Run, and contracted his disease there, on the second day of his illness removed to a relation's in St. John's Street, near to Callowhill, where he died on the fifth day from the attack,

with symptoms of the most malignant character. The disease was not communicated to any of the family.

Many other similar facts might be recounted, but which it is scarcely necessary particularly to enumerate. They are irreconcilable with the phenomena and laws of contagion. Dr. Haygarth has investigated that subject with great patience and assiduity, and it is to his researches, and those of Dr. Bancroft, that we are indebted for the only precise information we possess relative to contagion. As the result of his observations on typhus fever, exhibited in a series of tables, he found, that "five only out of one hundred and sixty-eight exposed to infection, remained uninfected, or less than one in thirty-three."* But in the last summer and autumn, more than two hundred individuals were exposed under the most favourable circumstances to the operation of contagion, did it exist; and of that number, not an instance occurred of the disease being contracted by intercourse, even the most intimate, with the diseased. The friends and relations of the sick, became their kind and faithful attendants, and the constant companions of their bed-side. I have seen wives with devoted affection, supporting in their arms, and soothing with caresses, the last moments of their dying husbands; children consoling with their attentions their expiring parents; and parents overwhelmed with grief, administering to the last wants of their departing offspring. In this triumph of affection over fear, the lover was seen refusing to abandon the object of his attachment, struck down with the disease, despising the warnings of the danger to be apprehended, and resolving to share with her a common danger and a common fate.

The performance of those sacred offices of humanity, and examples of the best and kindest feelings of the heart, it is gratifying to know, were on no occasion productive of ill consequences. The indulgence of them brought none into danger by the disease being contracted.

From the numerous occurrences of the kind that have been pointed out, the conclusion, it appears to me, is irre-

* Haygarth's letter to Dr. Percival, p. 82.

sistible, that the disease, last year, was not of a contagious nature.

The manner in which the disease broke out and prevailed is not consonant with any known laws of contagion. At Hodge's dock, most of the cases occurred simultaneously, and the whole number in the course of ten days. They were scattered over a space the two extreme points of which are about long pistol shot distant, and none of those who were sick, had communication with any one labouring under the disease, prior to their attack.

The same circumstances were observable at Walnut Street wharf. Several individuals were often seized on the same day, without having had an intercourse or been near any one who was sick, and more than half the cases took place in ten days, twenty-six having been attacked from the 6th to the 16th of August. Preventing access to that situation by barricades, immediately checked the increase of cases, although a number of persons who had derived the disease from an intercourse with it, were then lying ill in various parts of the city.

I have before remarked, that a similar state of things was observable at Duke Street and its vicinity.

Dr. Haygarth in his letter to Dr. Percival, examining into the distance to which febrile contagion extends, arrives at the following conclusion. "The whole evidence which I have been able to collect, incontestably leads to this very important conclusion, that febrile infection extends but to a very narrow sphere from the source of the poison."* The correctness of this conclusion, has been repeatedly verified, by observations in those fevers, which are unquestionably contagious. Dr. Chisholm is of opinion that "the distance may be fixed at the utmost at six or ten feet."† Now this law is completely hostile to the numerous facts, observed in the malignant fever of last year. Messrs. Smith, Ansley, Edwards, Barker, Nesbit, the Messrs. Kings, Jas. Forsyth, Mrs. Baird, Mrs. Duffy, Thompson, the Sprowles,

* Page 48.

† Essay on the Malignant Pest. Fever, p. 154.

Room, the Whartons, and in truth, nearly all those, who were affected with the disease, had not been near to any one sick, nor approached any suspected source of contagion.

The rapidity with which the malignant fever of last year spread, is also inconsistent and irreconcilable with the laws of contagion, as observed in contagious diseases. They are propagated in a slow and very gradual manner, from individual to individual, a fact recognised by Dr. Hosack in his pamphlet,* and demonstrated by Dr. Haygarth, in his tables. "Out of seventy-two cases," remarks Dr. Haygarth, "the latent period of the typhus (allowing four days of fever before the patient becomes infectious) was less than ten days in only five, or probably in only three cases: it was less than seventeen days in only eleven or thirteen: it fell upon some of the days between the seventeenth and thirty-third in forty-one, which is considerably more than half the cases."† Now, at Hodge's dock, we have seen, that the whole number of cases occurred in the space of ten days; at Duke Street, nine cases out of twelve, took place in four days; and at Walnut Street wharf, in more than half of the cases, the disease was also developed in ten days. No contagious disease, not even the plague, the contagion of which is of the most deadly and fatal nature, spreads with such rapidity, or is characterised with similar phenomena.

Although disposed to yield credit to the doctrine of the contagious nature of yellow fever, prior to the experience of last year, and the present examination of the subject, the testimony and the observations collected, are conclusive to my mind, that the disease, as it then existed (I speak not of former years), presented no evidences of contagion.

The importance of this subject may justify some additional remarks. There are two classes of febrile affections, proceeding from very different causes. The one is, without doubt, produced by contagion, and is propagated by a poi-

* P. 12.

† Haygarth's letter, p. 66.

son secreted by the system of those labouring under a peculiar disease. In this class is comprehended typhus, plague, small-pox, &c. The other class arises from a poison, formed by vegetable decomposition, under particular circumstances of heat and moisture, which being thus generated, floats in, or is diffused through the atmosphere.

Aut etiam suspensa manet vis aëre in ipso,
Et quum spirantes mixtas hinc ducimus auras,
Illa quoque in corpus pariter sorbere necesse est.

LUCRETIVS, Lib. vi. 1124.

Such are our autumnal intermittent, bilious and remittent fevers, the malarian fevers of the Pontine marshes near Rome, the endemics of the East and West Indies, the fevers of Walcheren, Beveland, and, in fact, of all marshy countries.

It would tend greatly to simplify the question of the contagion of yellow fever, as a preliminary step, to ascertain to which of these classes it properly belongs. If it can be established by the most indubitable testimony, that marsh miasmata not only give birth to malignant fevers of a similar character to yellow fever, but occasion yellow fever itself, the contested field will be greatly narrowed in its limits. That fevers exhibiting an equal malignancy with yellow fever, and closely partaking of its symptoms and character, are occasioned most unquestionably by marsh miasma, is a fact based and sustained on medical observations of such high authority, both ancient and modern, as to be placed beyond the reach of cavilling or doubt. It is a doctrine taught by the philosophic poet Lucretius,

aut ipsâ sæpe coorta
De terrâ surgunt, ubi putorem humida nacta est
Intempestivis pluviisque, et solibus icta.

Lib. vi. 1094.

Examples are furnished by the endemic fever that prevails throughout Bengal; the malignant intermittents of Italy, and those produced by the malaria of Rome; the fevers that rage in Hungary in the months of July, August and September, described by Dr. Kramer "as altogether the same with those that are epidemic upon the coast of Guinea;

and in the sickly climates of the East and West Indies ;”* the endemic fevers of the coast of Mexico and of the West India Islands ; of the coast of Africa, and of our southern states. These fevers are the unquestioned offspring of marsh miasmata ; are mostly of as violent and fatal a character as our yellow or malignant fever ; and some of them cannot be distinguished by a single symptom or circumstance from it. If, therefore, miasmata arising from vegetable decomposition can occasion fevers of a character similar in every respect to yellow fever, it assuredly gives a strong probability, that yellow or malignant fever may have the same origin. When we find it occurring under precisely the same circumstances, and observing the same laws, as the fevers originating from marsh miasmata, that probability assumes the character of certainty, and almost of demonstration. But we can go a step further, and make it manifest, that yellow or malignant fever has been produced, where no other agent than marsh miasma could be suspected of its production. Mr. James Johnson, in his excellent work on “*The influence of Tropical Climates*,” has furnished us with the fact. A number of the British troops sent in the expedition of 1800 against Batavia, were landed on the uninhabited and marshy island of Edam, and where of course contagion could not exist. They were immediately attacked with yellow fever. Scarce an individual who passed a night on the island, escaped the disease, and nearly all who were attacked with it died. The crews and soldiers who remained on board the ships, enjoyed their usual health ; and no instance occurred of the disease being communicated by those who were sick to their comrades. The yellow fever is well known also as the endemic of Batavia ; but its importation there has never been suspected, nor any other cause assigned for its prevalence, than the exhalations from its foul and pestilent canals.

If then any medical fact is to be credited—is established by testimony that cannot be shaken, it is, that fevers of the same grade and character, and of similar symptoms with

* Lind on the health of seamen, p. 55

yellow fever, are produced by marsh exhalations in tropical latitudes. We have it also proven in an equally satisfactory manner, that yellow fever has been produced by the same cause. How, then, has it happened, that a contrary opinion has prevailed, and yellow fever been supposed to be a disease originating from another and distinct source, and only propagated by contagion? From investigating this question, I can find no other authority for the opinion or ground on which it is sustained, than a notion of Dr. Chisholm, formed from a belief in alleged occurrences and circumstances, that have since been completely disproven, and which are now known never to have had existence.

In 1793 the yellow fever prevailed at St. George's in Grenada, to an extent and with a mortality that was uncommon at that place. Dr. Chisholm does not expressly inform us, whether a greater number of Europeans than usual, were at that time in port. When that circumstance occurs in the West Indies, yellow fever is always more general, from the mere fact of the increase of the number of persons who are subject to its attack. But although the Doctor has neglected that important point, he gives room to infer that there was an unusual accumulation of new-comers obnoxious to the disease. He states that two hundred and fifty seamen died with the disease, or "a third of all the sailors, during about ten weeks in harbour."* There must consequently have been in the port of St. George's about seven hundred and fifty sailors, independent of the troops of the garrison, many of whom, Dr. Chisholm states, were recruits, and were those of the troops who suffered most. We are not informed by the Doctor of the population of St. George's; but in Edwards's History of the West Indies, the white inhabitants of the island are stated to have been 1300 in 1777, and to be decreasing. Morse, in his Gazetteer published in 1804, computes the number at 1000. There must have been, therefore, collected in the small port of St. George's, in the months of March, April, May and June, the months when

* Essay, &c. p. 91. Edwards's W. Indies, vol. p. 74.

yellow fever always exists in the West Indies, sailors, recruits and other new-comers, all the subjects of the disease, equal in numbers to the whole white population of the island. Can it then be surprising, or so very wonderful, that the endemic yellow fever of Grenada should have become more than usually rife, when there was so great an increase of those who were subject to its attack, that we must resort for its explanation to the supposition of the creation of a new, unknown, and before unheard-of disease? Certainly not. It is the precise result that ought to have been calculated on, and which is known invariably to occur under the same circumstances.

But it was not at St. George's alone, that the yellow fever was more than usually prevalent in that year. Most of the islands—Jamaica, St. Domingo, Cuba, &c. witnessed its ravages: and our city was laid waste with its desolation. But where is a single fact recorded, or the slightest evidence to be found, tending to show that the disease was conveyed from Grenada to the other islands, and to this city? Yet such ought to be produced to countenance the conjecture of Dr. Chisholm.

That fevers usually sporadic, in certain states of the atmosphere will become epidemic, and that diseases will be suspended or disappear for a series of years, and again return suddenly, and prevail with violence, are occurrences by no means novel. They have been recorded from age to age, and it is scarcely possible to pursue the profession of medicine a few years, and not be familiar with the fact. When we know that these phenomena have been, and are repeatedly presented to observation, in intermittent, bilious, remittent and many malignant marsh fevers—in dysentery, influenza, &c. on what principles and on what grounds, shall we deny to the yellow fever of tropical and temperate latitudes, a similar character, or exclude it from the operation of laws universally applicable to other similar diseases?

If then the presence of a great number of individuals, obnoxious to the attack of yellow fever at the port of St. George's, in the months of March, April, May and June of the year 1793, is quite adequate to account for its

unusual prevalence there; or that the circumstance can otherwise be satisfactorily explained on known principles, and is in conformity with long-observed and well-established facts, the supposition of the creation of a new and before unknown disease, is unnecessary and unphilosophical. It is little better, than the absurd conjecture of Hercules Saxonia, that the plague is created by the spells of witches, who, whether alive or dead, he believed, were possessed of great powers for its production.*

But the opinion promulgated by Dr. Chisholm, that the disease of 1793 was a new disease, "before unknown in this country," and generated on board the ship *Hankey*, has been most clearly proven to be fallacious, and the facts on which he has pretended to support it, have been demonstrated to be fictitious. The evidence to these points is unimpeachable, and the testimony conclusive. It is furnished by Mr. J. Paiba, one of the adventurers in the Boulam scheme, and who was on board the *Hankey*;† by Capt. Beaver, who commanded the *Hankey*, in his African memoranda; by Dr. Winterbottom, the physician of the Sierra Leone company; by Mr. Smithers, the surgeon of the *Charon*, a ship which Dr. Chisholm asserts was infected by communication with the *Hankey*; and by Dr. Trotter, in his valuable and standard work, *Medicina Nautica*. The whole statement of Dr. Chisholm is proven by the above evidence to be little better than a romance. No such disease," as he asserts "afflicted the crew and passengers of the *Hankey*, was known on board of her. She arrived at Grenada with all on board in perfect health; and could not have communicated the disease attributed to her. The account by Dr. Chisholm of the disease of the *Hankey*, and the introduction of the yellow fever by her into Grenada, has been particularly examined by Dr. Nathaniel Bancroft, in the seventh appendix to his philosophical and valuable work on yellow fever. The editors of the *Edinburgh Me-*

* *Sagarum non tantum vivarum, sed etiam defunctarum magnas esse vires ad pestis inductionem. Hercules Saxonia, de plica.*

† *New-York Medical Repository, vol i.*

dical and Surgical Journal, in their review of that work, thus express their opinion on that particular subject.

“ We can only notice some of the leading points in his Appendix (No. vii.); the sole object of which is, to confute the statements of Dr. Chisholm, respecting the origin and propagation of the yellow fever, but especially as to its alleged importation from the coast of Africa to Grenada, in the ship *Hankey*.

“ In these statements, Dr. Bancroft has, in our opinion, proved incontestably, that considerable incorrectness exists in the details of Dr. Chisholm.

“ We profess our great respect for the learning and talents of Dr. Chisholm, (of both of which this Journal has exhibited many proofs); but “ *magis amica veritas*,” we cannot but yield our assent to the views of Dr. Bancroft, supported as they are in this work, upon the subject under discussion.”*

From this view it results—1st, That fevers produced by the poison designated as marsh miasma, are of the same character and grade, and possess similar symptoms with yellow fever: 2dly, That yellow fever has been and is occasioned by marsh miasma: and 3dly, That the opinion that has assigned to it another origin, and would place it with those diseases that are produced alone by peculiar contagion, it has been incontestably demonstrated, was formed on grounds entirely fallacious.

Yellow fever, therefore, must be considered as belonging to that class of diseases produced by marsh miasmatic poison; and the solution of the question, whether any of those diseases do become contagious when once contracted, will materially aid in deciding its character.

No investigation, that I know of, has been made directed exclusively to the examination of that point. But the general facts with respect to that class of fevers, are repugnant to the supposition that they ever do become contagious. No one at present suspects that intermittents, common bilious, or remittent fevers ever assume that character. Dr.

* Vol. viii. p. 340.

Chisholm expresses his conviction in the most decided manner, that the endemic yellow fever of the West Indies, "evidently caused by marsh effluvia, heat," &c. never is contagious. "I have never, in any instance," he observes, "and I have seen many, of yellow fever, known it to be contagious: it has always been evidently produced by the causes mentioned; (viz. marsh effluvia, heat, &c.) and other persons on board the same ship, or in the same house, have continued in perfect health."*

An extensive field of observation was afforded to determine the subject, by the expedition sent from England against Zealand in 1809. Two thirds of the troops were affected with the endemical, intermittent and remittent fevers of that country. More than 12,000 sick, labouring under those diseases, were transported to England, necessarily much crowded together in the transports, and were distributed in the different military hospitals. Yet, under those favourable circumstances for the production of contagion, it was the unanimous opinion of all the medical attendants, "that no patient having the Walcheren or Zealand fever, had, as they believed, given that disease to any other; and, that, according to their knowledge and information, none of the attendants, or others, employed in the hospitals, and who had not been exposed to marsh miasma in Zealand, were attacked with the fever in question.†"

Many thousand articles of clothing and bedding, that had been used by the sick, and were imbued with their excretions, were also sent to England, "without being washed, and mostly in a filthy condition." Yet none of the crews of the vessels in which they were transported, although the articles were kept on board "several weeks," nor any of those who were employed to cleanse them, or of those through whose hands they passed, became infected.‡

Dr. Lind, however, expresses an opinion, that fevers produced by marsh effluvia, may, by want of cleanliness, and through close confinement on board of ships, become con-

* Essay on Malignant Pest. Fever, p. 147.

† Bancroft on Yellow Fever, p. 303 and 307. ‡ Ib. p. 309, 311.

tagious. The ample experience and correctness of observation of Dr. Lind, which render his works of so much value, certainly entitle any opinion of his to be received with attention. But it is not improbable, that typhus, so frequent a visiter of ships, in the instances alluded to by Dr. Lind, was modified by the previous exposure of the crews to the effluvia of marshes, or that marsh fever was modified by the existence of typhus contagion on board the ship. The fact that diseases are thus influenced by each other, and their peculiar causes, is well established by repeated observation. Dr. M'Gregor, in his "*Medical Sketches of the expedition to Egypt from Bengal*," informs us, "that the cases of the plague, sent from the crowded hospitals of the 61st and 81st regiments, were from the commencement attended with typhoid or low symptoms."

"The cases sent from the Bengal volunteer battalion, and from other corps, when the army was encamped near El Hammed, were all of the intermittent or remittent type," and those that occurred in the winter, were inflammatory, and required bleeding.*

Lempriere also mentions, "that the common typhus, produced by causes existing in ships, or derived by them from places where it already existed," prevailed in the West Indies. "It was contagious at first," he remarks, "and acquired some of the symptoms of the tropical endemic, gradually losing its contagious property, the force of which seemed to be diminished by the climate."†

Dr. William Ferguson, inspector of military hospitals, in a very valuable paper, full of information on the subject of yellow fever, gives a most striking exemplification of the principle I have mentioned. "The fevers," he writes, "on board of her (the brig Childers, arrived at Barbadoes from Trinidad) from crowding below decks when at sea, ceased to be yellow ones, and became as truly typhoid as any I ever saw; but all that were taken ill after she came into harbour, [and] were promptly removed to our excellent hos-

* *Medical Sketches of the expedition to Egypt*, by Jas. M'Gregor.

† *Practical Observations on the diseases of the army in Jamaica*.

pital, retained the character of yellow fevers in every respect, and showed not the least of the typhoid type. That the ship was impregnated with a typhoid contagion, capable of infecting others within its sphere, I have little doubt.”*

Sydenham also mentions many examples of one disease wearing “the livery,” as he terms it, of another disease, yet retaining in its disguise its own essential character. A knowledge of this important fact, enables us to account for many anomalous circumstances in the history of diseases, that would be otherwise inexplicable, and places us on our guard against erroneous conclusions, deduced from appearances wholly deceptive.

Upon the whole, the facts and observations recorded by physicians of the best authority and most extensive observation, warrant the inference, that fevers originating from the poison commonly called marsh miasma, or more properly designated by the late Dr. Edward Miller of New-York, *koino-miasma*, never acquire contagious properties. And as yellow fever must be considered to belong to that class of fevers, the single attempt to prove it otherwise by Dr. Chisholm having failed, it would appear, *a priori*, to be proven, that it is not and cannot be contagious. This conclusion of legitimate deduction, fortified by the immense mass of facts and observations recorded of this disease, contradictory to all the known phenomena of contagion, and by the almost unanimous opinion of the physicians in this country and the West Indies, who are the most conversant with it, must be deemed, by every considerate mind, to possess the highest degree of probability. This is as near an approach to truth, as can generally be expected, in a science not susceptible of mathematical certainty.

* *Medico-chirurgical Transactions*, vol. viii. part 1, p. 152

(To be concluded in the next number.)

ART. II. *On the Medical Character of the United States.*

By S. COLHOUN, M. D.

THE winds, which blow over a vast wilderness on the North and West, and the Ocean on the South and East, subject the climate of the United States to perpetual vicissitude: the northern differ from the southern districts in having seasons of predominating cold, giving a character of excessive rigor to the winter, of change to the autumn and spring, of moderation to the summer according to the elevation of the latitude. The south, more affected by the sun, has a longer summer, a milder spring and autumn, and a shorter winter. The middle climates vibrate between these extremes, and the country beyond the mountains possesses a more moderate temperature, than that which borders the Atlantic Ocean.

The climate of the middle states exhibits in the spring, the characteristic moisture of the British isles, as its most general feature; often the burning and malignant sky of the tropical regions in the summer, the mildness of the climate of Barbary in the autumn, and sometimes the atmosphere of Russia in the winter; frost has appeared in every month; in summer fire is sometimes necessary; in winter occasionally disagreeable; garlic, a vegetable common on the eastern border of the country, has been seen in January; blossoms in December and in February, and in the same months, the temperature has descended to twenty-two degrees below 0 of Fahrenheit's scale. The spring is peculiarly variable, from the alternate influence of the sun and the winds from the north and west, sweeping across the continent between the inhabited regions and the Pacific Ocean: In general, there are thirty or forty days, in which the thermometer rises above eighty, or sinks below thirty degrees, measuring the extreme heat and cold of the year. Winter commences about Christmas and continues till the middle of March or beginning of April: As in the more northern districts of Europe, this season is diversified by high and tempestuous winds, heavy snows and rains, with sleet and hail

in various succession. A protracted autumn often suspends its rigor till the middle of January; sometimes it commences in October and incroaches upon the spring. The cold is often steady and regular, with a weak sun, cloudless and starry nights; the winds bitter and piercing from the north and west, the earth for weeks covered with snow, and the rivers and smaller streams with ice. April often raw, sometimes mild and showery, is the first month of spring. In May, the weather is for a few days intensely warm, and generally continues temperate till the middle of June. Summer then commences; the excessive heats of July and August are moderated by heavy dews, by occasional showers and storms, accompanied with thunder, lightning, and sometimes hail, succeeded by agreeable winds, varying from the north and west. The mornings and evenings become cool and the days pleasant in September; in October, the autumn is completely established; the country is in this season most attractive and agreeable; the fruits of the year abound; the trees are beautifully variegated, and instead of the sombre and melancholy drapery of European forests, they have a gay and enchanting appearance; the mornings and evenings gradually growing colder, the country dreary and solitary, a few weeks of mild weather with a smoky atmosphere, vary the month of November; as it advances, winds from the north chill the air, and foretell by the fall of raw and cold rains accompanied with sleet and snow, the approach of winter. These storms clear away; mild weather succeeds; the roads generally dry and fine, render travelling agreeable, till winter appears in all its rigor.

The winds from the north and west, rendered severe in winter, by passing over extensive regions of snow, descend over the inhabited territories, exalt the tone of the system, and produce inflammatory disorders. During their prevalence, catarrhs, quinsies, pneumonies, consumptions, colic, asthma, gout, rheumatisms, continued and intermittent fevers in an acute form occur. The eruptive and other diseases are then more inflammatory, and require more depletion. Common remittents become synochal, and intermittents change

into continued fevers; convalescence is more safe, and dropsy or typhus more rarely a consequence of violent attacks. The same winds, cooled in summer by traversing a vast wilderness, the surface of the lakes, the sources and streams of innumerable rivers, appear occasionally, tempering the violence of the sun, and lessening the malignity of disease. Those, which pass over Mexico and its bordering ocean, are relaxing and often accompanied by rain. In the east wind, cold and moisture combine to heighten the effect, and acquire their greatest power in the raw, chilling, and depressing currents, which blow from the north-east, over an extensive ocean. Diseases of a low type in this country, as in Europe, have been supposed to result from the prevalence of moisture; united with cold it renders inflammatory diseases more frequent and protracted, and less susceptible of depletion; with heat, the air becomes pestilential, ordinary intermittents degenerate into typhus, and continued fevers become malignant, followed by dropsy and the various forms of nervous debility.

The waters of the United States falling into the lakes on the north, into the Gulf of Mexico, on the south, find their way to the Atlantic, and produce disease, by the putrefaction of vegetable remains on the shores of rivers and arms of the sea, or by the formation of vapor on the surface of stagnant pools and marshes. The Alleghany, the principal range of mountains, divides the country into Eastern and Western sections, tempering the air of summer and increasing the rigor of the winter by the coldness of their summits.

The mode of living resembles that of Britain. Our food generally plain, sometimes luxurious, introduces in the cities of the middle latitudes, after the 4th of July, the festival of Independence, the diseases of summer; at Christmas, those of winter. The dress, particularly of the men, is adapted to the season; that of the women, sometimes light and fashionable, is a frequent cause of coughs, consumptions, rheumatisms, and other inflammations. The houses constructed for warm rather than for cold latitudes, are

rendered comfortable by the abundance of fuel. The character of the people varies in different districts. A mind highly enlightened, adventurous, persevering and vigorous, from the necessity of exertion imposed by a sterile soil and a cold climate, distinguishes the North—moderation and industry, the middle states—enterprise, the prevailing quality of every region, receives in the South a characteristic shade from the moral constitution of society, from habits of luxury awakened by a genial climate, and strengthened by the abundant productions of a fertile soil—whilst the west, towards which the tides of emigration are continually setting, displays, on a vast shore, fragments of population from the various nations of Europe, and districts of North America.

A general view of the origin of diseases, will expose their comparative state in Europe and this country. Heat and moisture, and their contraries, are the causes evolved by climate and situation. In the North, at a distance from the ocean and great bodies of water, where the air is cold and dry, where from the weakness of the sun and the unfruitfulness of the soil, incessant labour is necessary, the tone of the system becomes exalted, and the diseases are phlogistic in their type; accordingly, quinsies, pneumonies, consumptions, rheumatisms prevail; convalescence is more rapid, dropsies, nervous diseases, obstructions of the liver and spleen, disorders of the alimentary canal and bilious fevers, are more rare. The winter season and mountainous exposures, particularly on their northern aspects and in the young and laborious, develope this diathesis more fully, than the vallies and lowlands, sequestered from winds; in the former, an epidemic catarrh often becomes a pneumonic inflammation, intermittents change into remittents, remittents into synochal; whilst in the latter, they run into low nervous fevers followed by a long convalescence, particularly among those who are debilitated by age, intemperance, or any natural or adventitious cause. In less elevated latitudes and exposures, lying near the ocean, or broken by rivers, lakes and marshes, when cold and moisture prevail, the system, lowered in its tone, is still subject

to inflammatory diseases and fevers, which sooner terminate in typhus, are more frequently followed by dropsy, jaundice, chronic disorders of the viscera and of the nervous system, or a slower recovery than in more elevated, arctic and dry exposures: Tertians and fevers generally become obstinate, and degenerate into typhus, particularly in autumn and spring, when cold and humidity are united in the highest degree, and in those whose systems are defective in tone from premature or advancing age, from intemperance, poverty, or any debilitating cause. In latitudes and situations where the vicissitudes of the atmosphere are great, insanity is supposed to occur more frequently; a circumstance perhaps more properly referred to moral causes, which have been particularly observed in the highly civilized communities, which occupy those latitudes where the changes of the air are sudden. In hot and moist situations and seasons, the prevailing diseases are malignant, pestilential and deadly; bilious fevers, cholera, colic, dysentery, diseases of the liver and spleen, succeeded by various and protracted forms of debility, by atrophy, consumption, hypochondria, palsy, syncope, jaundice, dropsy or obstructions of the viscera occur; and when moisture is excessively prevalent, the lowlands, which were formerly the extensive seats of vegetable putrefaction, are covered with water and become healthy, whilst the dysentery and fevers occupy the hills and more elevated country, which in dry seasons were entirely free from disease. The summers of the north, sometimes, of the south generally, exhibit the diseases produced by this combination of causes. The cold banishes them from the higher and mitigates them in the lower latitudes. Heat and dryness raise the tone of the system; under their influence the diseases of summer become inflammatory; remittent bilious fevers, cholera, dysentery and colic are common, but without the usual consequent debility and disorders of the viscera. On the southern exposure of mountains these causes are aggravated, and in a country whose surface is variously diversified, the dysentery is found to occupy its higher, and the remittents of the summer its

lower ranges : Heat alone is a cause of disease ; ophthalmia, epistaxis, fever, insanity, apoplexy, and gutta serena, are sometimes the effect of an intense sun, and cold often produces erysipelas, ophthalmia, and when excessive, insanity ; cold drinks have produced nephritis, gastritis, bilious colics, fever, and in the predisposed by former habits, gout. In making a comparative estimate of the diseases of the United States and Europe, arising from situation and changes of the air, let it be recollected, that the effects of the east winds in Europe, correspond with those of the west in the United States, and those which blow from the south, in both continents have nearly the same character. Moral causes are no less extensive in their agency. Religion and law, by encouraging virtue and industry, by repressing indolence and vice, have a direct effect upon the diseases of every community. Cities, towns and villages, according to their extent and the habits of the people, become frequent resorts of disease from defect of ventilation, from uncleanness and poverty ; circumstances immediately referable to moral causes. Prisons, alms-houses, hospitals, barracks, manufactories and ships, where great numbers of people are assembled, are also the seats of many maladies. The banks of rivers, exposed by the recession of the tide to the sun ; close, narrow and filthy streets, vegetable and animal remains, in a state of putrefaction, unite to render cities unhealthy, particularly in the summer and in southern latitudes. Watching and study, and the more violent passions, predispose to the various species of fever, in situations favourable to their generation, and are often the original causes of phrenitis, insanity, apoplexy, epilepsy, syncope, chorea, palsy, hysteria, hypochondria, dyspepsia, atrophy, consumption, gout, and occasionally gutta serena. Indolence and sedentary habits render women more susceptible of fluor albus, amenorrhœa, hysteria ; the men of hypochondria, and produce in both sexes mania, consumption, hæmorrhoids, obesity and apoplexy. Intemperance predisposes to the ordinary fevers, and is the direct cause of the various species of nervous debility, of palsy, hysteria, and hypochondria ;

dyspepsia, gout, atrophy, consumption, diabetes and dropsy, are also its consequences. The accidents of life greatly increase the catalogue of diseases in highly civilized communities: The various fractures and local injuries, the result of violence; the inflammations which are produced by mechanical and chemical causes, in manufactories and the diversified employments of extensive societies; the deformities of structure, multiplying difficult labors among women, and chronic diseases, in both sexes, which arise from habits of luxury, confinement, poverty and vice, must be comparatively rare in a sparse population. Those maladies, also, which are hereditary or contagious, and their consequent affections, extend more widely in thickly settled countries: thus syphilis, as a cause of predisposition to rickets, scrofula and other disorders; rickets as producing hydrocephalus; scrofula, as the cause of the same disease, as also of atrophy among children, and consumption among adults, must be comparatively limited in America; where climate and exposure have a more extensive effect than population or moral causes in producing disease: accordingly, the following sketch, with the application of these general remarks, will exhibit the medical topography of its various districts.

In the north, acute and chronic inflammations, more particularly catarrhs, quinsies, pleurisies, consumptions and rheumatisms, continued and intermittent fevers, occur: in the south, malignant and bilious fevers of various types, diseases of the liver, inflammations and spasmodic affections of the alimentary canal; in the middle States both, according to the season and exposure. The yellow fever visits the cities of the Atlantic coast in the summer, the typhus in the winter; increased by the evils and privations of poverty, sometimes inflammatory, epidemic and desolating, often sporadic and manageable, they personate the various forms of disease and even health, conducting the patient to the grave, without a suspicion of his danger. In general, malignant and bilious fevers appear in summer and the beginning of autumn, inflammatory and typhous affec-

tions in the winter and spring of all latitudes. The eruptive fevers occur at the interval of three or four years. Vaccination is general ; variola, subdued as far as the prejudices of ignorance and poverty will permit, sometimes appears in the cities ; varicella, scarlet fever, measles, erysipelas, have nothing peculiar ; the plague is unknown ; the miliary fever and pemphigus are seldom seen ; cholera frequent among children, more rare among adults, aphtha, colic, diarrhœa, dysentery, appear in summer ; the latter, sometimes epidemic, and in situations generally healthy, and appearing on the hills and more elevated country, when remittent fevers are seen in the vallies, is often extremely mortal. Urticaria appears in the adolescent, seldom in those of mature age—The various species of hæmorrhage exist here, as in Europe, and are known in all districts—that from the lungs, the harbinger of consumption, the endemic of the northern and middle regions, is comparatively rare in the south : the diseases peculiar to females, and those which result from the relations of the sexes, are more rare than in Europe, from the prevalence of early marriages, and the moral character of the people.

Apoplexy, palsy, and the various species of nervous debility, are often produced by intemperance, particularly in the south and west, as also in the cities, and destroy the aged towards the conclusion of summer and winter ;—active in our habits, with few men of leisure and overgrown wealth, hypochondria is more rare in the United States than in Europe.—Tetanus sometimes occurs in summer, seldom in winter ; in the middle States it is dangerous to receive a lacerated or a punctured wound during the warm season, without the use of local and stimulating applications ; in the south, it occurs, from wounds, as well as exposure to the night air—Tympanites is seen—epilepsy, chorea, syncope, and hysteria, are believed to occur less frequently here than in Europe ; asthma, pertussis and hydrophobia have nothing peculiar—Indigestion and the gout, often the result of intemperance, are not unfrequent in the southern districts of the country, or in the cities of the Atlantic coast.

In the north, the people contending with the difficulties arising from a dense population, with minds highly tempered by devotion, are subject to mania, from reverses of fortune, and the effects of religion. The same causes are rendered more powerful in the southern and middle States by a more ardent temperament, and more luxurious habits. There religion, vibrating between the extremes of enthusiasm and licentiousness, often passes like a contagion among the people, and developes in extensive assemblies excited by the fervor of public worship, the diseases of the nervous system. Mania, more frequent among men than women, is less common here than in Europe, from the moral character of the people, and the mildness of our policy. Older nations, burdened with the machinery of power in its most complex and elaborated state, liable to the great succussions and agitations of the moral world, produce in hospitals, almshouses, and cities, in fleets, in armies, barracks and other great assemblages of people, the most debilitating and exhausting vices of the race; or overspread whole districts with war, famine, pestilence, and the various evils which attend the great contentions of pride among the monarchs of the earth; giving origin in an extensive manner to the diseases of the mind, and every species of nervous debility. The various forms of dropsy are less frequent than formerly: more manageable where the constitution is not absolutely broken, they are confined to the aged, the poor, and the intemperate, or are the result of other diseases—Goitre is endemic in various situations, and cretinism is sometimes seen—Rickets occur, though rarely, among the children of the negroes—The scurvy is unfrequent—Jaundice is sometimes known—Scrofula is more common, occurring in glandular swellings, and in ulcers with caries—Psora is confined to the poor, and incurable cases of herpes sometimes occur: All diseases arising from uncleanness, or from unwholesome diet, are less frequent in this country than in the old world, from the greater abundance of provisions, and the superior habits of the lower classes; with perhaps the exception of those which proceed from intem-

perance, which in America certainly prevails to a disgraceful extent. The appearance of surgical diseases, is regulated, as in Europe, by the occupations of society, by the accidents of life, the decay of nature, or the peculiar susceptibilities of the system : Composed of nearly ten millions of people, scattered over an extensive territory, actively engaged in agriculture, with manufactures little advanced, our cities, army and navy small, the apparatus of national power only partially developed, the affections produced by accident, by sedentary, luxurious, confined or vicious modes of life, must be comparatively rare. The ocean, great rivers, marshes, highlands, mountainous regions or extensive plains, diversify the character of disease, according to the principles already detailed ; epidemics, for a time phlogistic or typhous in their temperament, occasionally desolate districts long blessed with health ; some situations are always afflicted ; others, in all seasons and in all years are undisturbed by any prevailing malady.

The duties of the profession are performed by physicians, surgeons, accoucheurs and apothecaries ; in the country the different branches are united ; in the cities they begin to separate. In the north, the profession is not well rewarded ; in the middle states it connects the law and divinity with the other occupations of society ; in the south it is more distinguished, and partakes in all districts of the prevailing character of the people. In the north, more learned, moral and judicious, the physicians incline to the practical duties of religion ; in the south, frank, open, and gentlemanly, regulated rather by honor than by more stable principles ; in the middle states they bear a character between these extremes.—In general the profession has been supposed to incline to scepticism : The constant intrusions of business, the influence of medical theory, sometimes openly inculcating materialism, the contemplation of physical phenomena, apart from the influence of mind, or the great relations of the moral world, estrange the profession from the duties of religion and expose it too often with apparent justice, to the charge of infidelity. The phy-

sicians of the country, instruct their pupils in the practice of medicine, therapeutics, and the business of the apothecary. Collecting from every quarter of the United States, they complete their education at the universities, by the study of anatomy, medicine, surgery, midwifery, therapeutics and chemistry. Collateral branches are sometimes added: in general, medicine, anatomy and surgery are considered as indispensable. Societies are formed in most of the states to improve the science, promote emulation, or regulate the practice. From the extent of the libraries, hospitals, almshouses, and the facilities of prosecuting anatomy, the universities are generally placed in the cities of the Atlantic coast; already New-Hampshire, Rhode-Island, Massachusetts, New-York, Pennsylvania, Maryland, Virginia, Kentucky and Ohio, have founded medical institutions; of these, the University of Pennsylvania is the most distinguished. Supported by a long established reputation, by the ability of its Professors, by the celebrity of the Pennsylvania Hospital, and its library, by anatomical collections, a great museum of natural history, and the infirmary connected with the alms-house, its pre-eminence is founded on extensive advantages. The results of American Medicine, consisting principally of practical details and descriptions of natural history are recorded in the transactions of philosophical societies, in elementary and periodical works, essays and lectures. In this extensive suburb of the scientific world, the period for its full display has not yet arrived. It is only in those ages of the progress of nations, when agriculture, manufactures and commerce have accumulated immense wealth, and extended the reign of luxury; when famine, pestilence, and war have multiplied the causes of death, that medicine established upon the mineral, the vegetable, and the animal kingdoms unites with other more distinguished sciences, to give dignity and effect to the purposes of power, by discovering from an enlarged survey of nature, the means of alleviating the pains, increasing the comforts, or extending the pleasures of life.

ART. III. *Cursory Remarks on a disease vulgarly called Milk Sick.* By WILLIAM W. LEA, M. D.

IN a particular part of the state of Tennessee, a febrile affection, of a most curious nature, is occasionally met with, which is of very rare occurrence. By the vulgar it is called the *Milk Sick*. So far as my information extends, this has not been often observed, nor except in particular situations. No one has yet, I believe, given a rational or satisfactory account of it. My knowledge of the disease is derived more immediately from that part of Tennessee adjacent to Alabama, and west of Cumberland Mountain.

This majestic chain runs obliquely across the state from north-east to south-west, dividing it into East and West Tennessee. The eastern end of the state is a rough mountainous country and extremely healthy—intermittent and remittent fevers being rarely seen except upon the larger rivers. Here also the *Milk Sick* is unknown. Part of the western end of the state, adjacent to Kentucky, is equally broken and enjoys an atmosphere not less salubrious.

As we proceed further to the south and west, the face of the country becomes quite level, and the soil extremely fertile. Vegetation in some places is incredibly luxuriant, the lofty forests and impervious undergrowth entirely excluding the rays of the sun, so that, in warm weather, moist exhalations, impregnated with noxious effluvia, are disengaged in great abundance.

Dispersed over the country are numerous ponds, especially near the mountain. From the flatness of the surface, the waters run off slowly, and in some places are quite stagnant. These causes combined, aided by the summer heat, become the prolific source of fevers of the intermittent, and remittent type, sometimes in a very aggravated form.

Franklin county stretches along the foot of Cumberland Mountain 30 or 35 miles. This county is watered by Elk river and its branches. To the north and north-west of this

lies Bedford, watered by Duck river and its tributaries. These rivers, more than any others in the state, are remarked for producing the species of fever above mentioned. This very hasty sketch of the medical topography of the country I have given, as it may throw some light on this enquiry.

As soon as settlements commenced in the county of Franklin, about twelve or fifteen years since, near the mountain, many cattle were lost from some unknown poison, the nature of which is still a mystery among the inhabitants. Occasionally, whole herds were found dead in some sequestered cove of the mountain. Attention was directed to the investigation of this matter, and subsequent observation has led to the following results.

It has been found that the poison, whatever may be its nature, is confined to certain spots at or near the foot of the mountain, in those coves which have a western or north-western aspect. Those which open and look to the south are free from it.—The existence of the poison is of periodical occurrence, continuing from June to October. No cases of poisoning from this source have been observed before or after these periods. It appears, also, to have greater virulence in August and September than earlier or later.

If cattle remain on those contaminated grounds during the night, or seek them early for their morning food, they always suffer more or less from the poison. But after the sun has risen, so as to dissipate the dews, they feed in those places with perfect safety. With the knowledge of this fact, many of the farmers were in the habit of *penning* their stock at night, and until nine or ten o'clock in the morning, when they were turned out to range, without the hazard of poisoning. But within a few years, a fence has been extended for many miles along the foot of the mountain, so as to exclude this nuisance—in consequence of which, cases are of much more rare occurrence than formerly.

The depredations of this insidious enemy are not, however, confined to the cattle. Not a few of the inhabitants have been its victims. Generally those who have suffered

from the cause, are supposed to have been injured by the flesh, milk, or butter of the animals, that had previously taken this deleterious agent, and in which it had not manifested itself with sufficient violence to attract attention. Hence the popular name of the disease.

That such is the origin of this affection I have some reason to doubt; though, so strong is common opinion to this effect, that it may seem to argue no little scepticism to call it in question. This however should not shield error from scrutiny. It certainly affords a most curious and interesting subject for speculation. But, be this as it may, there is less uncertainty on another point. Men may be infected as other animals by similar exposure. Lying on the ground, in the poisonous tracts, or remaining there for several hours during the night, is always followed by an attack of this disease, which has occasionally been fatal.

In men, the disease thus induced is a gastritis, with some modification of the usual symptoms accompanying this affection, as produced by miasmata generally. The stomach is extremely irritable, the bowels torpid and obstinately costive, with great febrile excitement and determination to the head. There is, also, a peculiar odour emanating from a patient labouring under this disease, more especially as death approaches, which is, perhaps, the most striking diagnostic. But for this, it might sometimes be more difficult to distinguish it from the more violent attacks of bilious remittent fever.

I am not sure that I have ever had under my own care a genuine case of this kind. I have frequently met with very aggravated cases of bilious fever in places where the milk sick was common, and which the friends of the patients suspected to be of this latter character. I have even seen cases attended with black vomit, but the peculiar odour was wanting.

Of the treatment, I can of course say nothing from experience. The popular practice, and that which has been adopted by physicians, is to purge actively. To open the bowels is the *sine qua non*: but to effect this is always ex-

tremely difficult. The most incredible doses of calomel and ol. ricini are sometimes given, aided by injections, without any effect. Whenever an evacuation is obtained, immediate relief, in some degree, is the consequence. This is to be followed up by active purgatives. Blisters, diaphoretics, warm or cold applications as one or the other seems to be required, and occasionally the most powerful stimulants are employed with advantage. Convalescence is generally tedious, and relapses frequent, even at the distance of twelve months. The hair, epidermis, and nails sometimes drop off, and some constitutions never recover from the shock.

I do not know that *post mortem* dissections have been made, of the human subject. The stomachs of brutes show in places marks of inflammation, and some of the ventres are said to have the appearance of being broiled, or contracted by heat.

A considerable variety of opinion has prevailed as to the nature of this poison. Some have supposed it vegetable, others mineral. Mushrooms, a species of weed bearing a black berry, and some other vegetable substances, have been suspected, though without evidence. The waters have been accused of this mischievous agency, from mineral impregnation, but this is a merely gratuitous conjecture. There are even some who think it a mineral exhalation from imbedded ores, imbibed by the dews as it ascends. Any one who knows the general fixed nature of the mineral poisons, will readily perceive the absurdity of this hypothesis. None seem to have a definite idea on the subject.

My own opinion is, that it is a miasmatic exhalation. This I confess would be a strange notion to the people in that part of the country, but may not be the less correct for its novelty. When the profession, even, know so little of the nature and phenomena of miasmata, it will not be expected that others should understand, or be able to appreciate any reasoning on this subject.

During the summer months the heat ranges, at some time in the course of the day, from 80 to 90 of Fahrenheit. The nights are very cool, so that there is frequently in the twenty-

four hours a difference of temperature of 20 or 30 degrees. The excessive heat of the day produces copious exhalations of noxious effluvia, from the ponds, rivers, marshes, and forests—so rarified that they are productive of little injury. When night approaches they become condensed by the colder atmosphere, and from increased gravity begin to subside.

Vapours visible, and invisible, are attracted by mountains, and other elevated objects. In addition to this, there is commonly, at this season of the year, in the evening, a western breeze, by which the nebulæ, and vapours are gently wafted towards the neighbouring mountain. From its height they are impeded in passing over, and being driven into the recesses and becoming still denser, the more ponderous particles gradually glide down the declivities, which rise in amphitheatrical grandeur. The vortices in the centre are the hot beds of the poison.

Here, then, we may readily conceive that these deleterious miasmata, which, when rarified, produce the common autumnal fevers, have now acquired a degree of concentration and virulence, sufficient to produce even a more violent disease than the milk sick—and when taken in along with the copious dews deposited on the herbage, are fully adequate to the destruction of whole herds of cattle.

That miasmata do occasionally acquire a degree of force and concentration sufficient to produce disease in brutes cannot be denied. The instances on record are numerous. During the prevalence of the malignant yellow fever at Cadiz in the year 1800, “the disease spread to domestic and other animals, and dogs and cats were seen dying with black vomit. The very horses died!”

With these facts, we readily understand why those coves or recesses which have a southern aspect are exempt from this mysterious poison—and, also, why after the sun has dissipated and rarified the dews and vapours, they are no longer noxious. I should, perhaps, have stated that there are, I believe, in the vicinity of all those contaminated districts,

to the westward, ponds or marshes of greater or less extent, which are the fruitful sources of autumnal fevers.

P. S. Since having written the preceding very imperfect account of this disease and its causes, I have received an interesting communication from Doctor M. S. Dixon, of Winchester, Tennessee, on the subject, who has seen several cases, and speaks from his own observation. Among other facts which had escaped my recollection, he informs me it has been ascertained that cultivation of the poisonous tracts destroys their noxious quality. If this be true, it would appear that the miasm emanates immediately from those places where the poison is received.

Doctor Dixon is also of opinion that the common belief respecting the production of the disease by the flesh, milk and butter of animals, is well founded. In the cases that have come under his notice, there was less febrile excitement than is said generally to accompany such attacks. The appearances, as he describes them, resemble very much the operation of a large dose of emetic tartar. Though heretofore involved in Cimmerian darkness, we may expect that future investigation will throw additional light on this subject, and that, by its elucidation, some interesting facts will be developed respecting the laws and habitudes of miasmata.

A Description of the Anatomy of the Urinary Bladder, with some of its appendages, as concerned in the operation of Lithotomy. By WILLIAM ANDERSON, Esq. Licentiate of the Royal College of Surgeons, Edinburgh. In a Letter to Professor Chapman, M. D. &c. &c.

SIR,

I HAVE the honour to transmit, for your observation, a description of what I have found to be the Anatomy of the

Urinary Bladder, with some of its appendages, as concerned in the operation of Lithotomy; and am sensible, from the discussion which has occupied the profession in this city, during the past year, relative to this structure, that you will be interested with any detail, that may tend to add to the information already possessed on this subject.—Much of what I shall lay before you, is the result of frequent dissections, made in conjunction with Dr. A. H. Stevens, one of the surgeons of the Hospital at New-York, during my stay there, in December and January last; and which was afterwards demonstrated by Professors Post and Mott, to their classes in that university; since which time, I have repeatedly, in the dissecting room of Dr. Harlan, of this city, shown, what I think to be a new view of the parts connected in this operation.—That this description might be assisted, I also forward a drawing, from a lateral view of the pelvis: all which, if you see fit, you may please to have inserted in the Medical Journal under your superintendence.

In conducting this description, I shall sketch generally, the parts concerned in lithotomy; and be more particular upon such of them, to which I would especially lead your attention.

Being prepared to take a view of the contents of the male pelvis, from the cavity of the abdomen, and having inflated the urinary bladder, we see this viscus rising considerably above the symphysis pubis, and occupying the anterior part of the space encircled by the lineæ-ileo-pectineæ: behind it, and a little to the left side, we also perceive the rectum, commencing from the sigmoid flexure of the colon, at the base of the sacrum; and taking a course, along the concave surface of that bone, and behind the bladder, to terminate at the anus.—We find the peritoneum, coming off from the anterior abdominal parietes, some way above the pubis, to get connexion with the bladder, to which it is found in close attachment; commencing at the apex of the fundus, and continuing down its sides and backwards.—This membrane is observed leaving the bladder late-

rally; to lie upon the iliac vessels; and having covered the *facia iliaca*, it proceeds to line the walls of the abdomen. At the posterior part of the bladder, the peritoneum remains attached, until it reaches the extremities of the *vesiculæ seminales*; but between these two bodies, it continues its adhesion to the bladder, for about an inch towards the prostate gland: It then altogether leaves the bladder and comes in connexion with the anterior part and sides of the rectum, at some distance from the extremity of the gut. It can now be followed, from binding the rectum to the sides of the sacrum, upwards to where it takes the name of *mesocolon*. Thus, we have the bladder, anteriorly, from the fundus downwards, totally devoid of a peritoneal investment, yet connected to the sides of the pelvis by a loose reticular tissue, occasionally interspersed with fat.

By separating the peritoneum from the iliac and *psaos* muscles, we observe the iliac vessels, taking a course in their way to the crural arch, along the sides of the bladder, a little to the outer margin of the brim of the pelvis, and lying upon the *facia iliaca* where it covers the *psaos* muscle. These vessels where they exist, under the appellation of common iliacs, are crossed by the ureter, that takes a passage over them in its route to the pelvis; this duct passes behind the peritoneum, to terminate in the bladder, in a situation afterwards to be pointed out. We also see the *vas deferens*, ascending the pelvis, by the side of the bladder, where the peritoneum has its line of attachment, and passing over the iliac vessels, finds its way to the internal abdominal ring, to take a part in forming the spermatic chord.

We have in view throughout a considerable extent, a tolerably dense, though semi-transparent membrane, immediately covering the *iliacus* and *psaos* muscles; from which it has been named, the *fascia iliaca*. This is seen, at the inner side of the crista of the ileum, continuous with a fascia, lining the *transversalis abdominis*.—After covering these muscles, and lying under the iliac vessels, it proceeds over the brim, and lines the upper part of the *obturator internus*, and side of the pelvis. It soon, however, takes its leave from the walls

of the pelvis, at a semi-elliptical line, convex toward the abdomen; commencing at the lower and inner part of the symphysis, proceeding upwards, on the obturator muscles, but below the hole, for the passage of the obturator vessels; then downwards and backwards, and terminating at the spinous process of the ischium.—From this line then, the fascia iliaca takes its course inwards, to find connexion with the side of the bladder, and adheres to it in a similar direction, as that, by which it departed from the sides of the pelvis.—Between these two situations of attachment, the loose portion of fascia, which Dr. Stevens and myself have taken the liberty to call *ileo-vesical*, is in width, anteriorly, about half an inch; it becomes gradually broader, until it is about an inch and an half, opposite the hole, for transmitting the obturator vessels; and then becoming narrower, is, at the spinous process of the ischium, less than an inch wide. It appears at first view, to be of a tendinous structure, and very different from the fascia iliaca properly so called; but this is owing to a circumstance, which does not dispute its continuity with that fascia; and which will be afterwards explained. The ileo-vesical fascia, at its anterior connection with the bladder, passes down to the upper part of the membranous urethra, under the arch of the pubis, and forms on each side a pouch of half an inch in depth, capable to receive the end of a finger. Thus we find it at its fore part, commencing its attachment to the side of the bladder, evidently above the situation of the prostate gland; and, in addition to this, the paraboloid course of its connexion with the bladder, is continued upwards, going toward the termination of the ureter; as will be seen by the plate; then downwards and backwards, to end at the extremity of the vesicula seminalis, where it is met with by the reflected angle of the peritoneum, in its passage to the rectum.

From this aspect, we can see, between the line of the attachment of the peritoneum to the bladder; and, that made by the fascia; the muscular coat of this viscus, covered by a thin membrane, which is a lamina, sent up by

the ileo-vesical fascia; and it is by it, that this portion of the bladder is supported, and made equally strong with that defended by the peritoneum.—This lamina, by sending a process anteriorly from the bladder, to the posterior part of the symphysis pubis, forms an anterior ligament, to the bladder, which also becomes a partition by dipping down between the anterior attachments of the ileo-vesical fascia, and fixing itself to the upper part of the membranous urethra, aids to form the two pouches before spoken of.

We shall now trace a dissection by the perineum.

Having placed the subject in the situation directed, for the operation of lithotomy, raised the scrotum and distended the rectum; we dissect away the common integument; this is thinly supplied with fat, as it lies over the bulb of the urethra and edge of the anus; but it has that substance amply afforded to it, where it is placed about the tuberosities of the ischia, so that we have it there sometimes an inch in depth, in what otherwise might be called a lean subject. We now observe, the superficial fascia of the perineum, extended over this, in common with the other parts of the body; but which, although thin, where it is seen coming up from the scrotum, is much thickened and condensed as it proceeds over the perineum, towards the ischia and glutei muscles: in which situation it is much more firm, perhaps, than at any other part of the body. This also having laid aside, we have brought under view, a flat surface consisting of the muscles of the perineum, connected together by reticular membrane. Much care is now required, with the scissors and forceps, to clean these parts, in order that these muscles and their relations to each other, may be distinctly seen. This having been neatly performed, the eye is taken with a white tendinous spot in the middle of the perineum, anterior to the anus; this is the centre of union of all the muscles of the perineum, except the *erectores penis*; and it is a process from the lower edge of the triangular ligament of the urethra; and distinct from the muscles at this part, otherwise than by affording them insertion: it is not a movable body, but nearly as firmly fixed as the bones of the pelvis

themselves. Now, having glanced at the *erectores penis*, running upon the *crura*, from the tuberosities of the *ischia*; we take a view of the *acceleratores urinæ*, the connexions of which I shall attempt to describe. We observe these muscles to commence their origin, tendinous, at each side: from the *ischium* under the *crus penis*, also from the *ramus* of the *pubis*, as far nearly as the *symphysis*: and where the *crura* join, fleshy fibres are seen to arise, from the outside of the *corpus cavernosum*. All the fibres, soon becoming fleshy, run downward and backward, parallel to each other, and being loosely connected to the parts underneath, meet their fellows, at acute angles, pointing toward the *anus*, to be inserted into the ligamentous point of union of the muscles of the *perineum*, and into a white tendinous line, of about two inches in length, which is their own production, and situated along the middle of the lower part of the bulb of the *urethra*.—Thus we have the posterior edges of these muscles, surrounding the extremity of the bulb,—lying upon the triangular ligament,—and not in a situation to have any attachment to the membranous part of the *urethra*, as has been stated by some anatomists.

We now remark, the anterior fibres of the *sphincter ani*, also inserted into the central tubercle, proceeding from the triangular ligament: and we moreover see, the *transversales*, crossing the *perineum*, from the inside and a little way up of the tuberosities of the *ischia*; in a direction as their name implies: and after joining each other, are inserted into the central ligamentous projection, within the situation of the former muscles. They are not placed however in a direct line with each other, but form an obtuse angle, pointing downwards and forwards, toward the bulb of the *urethra*. The action of these muscles, have been said by anatomists to effect a dilatation of the bulb; but this cannot be, inasmuch as they have no connexion with the bulb, but through the medium of the *acceleratores urinæ*.—It would appear, that the projection from the triangular ligament, which becomes the centre of union for the muscles of the *peritoneum*, might be elongated, by the continued

contraction of the *acceleratores urinæ*; as, that in time, these muscles would cease to have proper effect upon the bulb, by being deprived of a fixed point to act from. From this we can now imagine, the use of the *transversales*; they, by their manner of attachment, become the antagonists to the *acceleratores*, and, however paradoxical it may seem, their assistants at the same time.

We will now detach the accelerator of one side, from its origin, the which, by turning over, we shall expose the bulb; we find this muscle to have been easily separated, but at the ligamentous line of union with its fellow, to be firmly connected to the urethra, throughout its whole insertion.—At this stage of the dissection, we are made acquainted, that the *corpus spongiosum urethra*, forming the bulb, terminates abruptly upon the anterior surface of the triangular ligament, receiving from it a membranous sheath, that first attaches the bulb close to the ligament, and then proceeds forwards upon the urethra, under the *acceleratores* muscles.

Now, clearing the perineum, by removing the penis, we have a clear view of the triangular ligament; this, we find, does not go up to the very angle of the pubis, that part being occupied by what has been called the pubic ligament, which is placed in the angle for about half an inch deep, and from being very firm and dense, may be said to be made up of the inter-articular cartilage of the symphysis: the triangular ligament, we see stretched across the arch, from the *rami* of the pubis, and is connected above to the middle of the lower part of the pubic ligament; by which a space is left, on each side, for the passage of the internal pubic arteries and veins. This ligament, from being about an inch and a half in depth, has its lower edge semilunated, and formed into two arches, both sprung from the projection, which is the centre of union for the muscles of the perineum: they are concave facing the *coxyx*; and go towards the tuberosities of the ischium of each side, by a process firmly attached to the inner edge of the bone, and which terminates, becoming incorporated with the anterior sacro-sciatic ligament. It is by this process, that the internal

pubic artery is more or less concealed, as it ascends the ramus of the ischium; and, it is also an extension of this part of the ligament, running up the inside of the pelvis, that constitutes the aponeurosis of the obturator internus muscle. The projecting point, at the centre of the lower edge of the triangular ligament, then, is the part I have so often mentioned, as giving insertion to the muscles of the perineum: and this is kept in its situation, to give form to the arched appearance of the ligament, by the contraction of the sphincter ani and transversales perinei muscles. We observe also, in the middle of the triangular ligament, a hole, which gives passage to the urethra from the bulb.

Now, removing a large quantity of fat from a pit found between the side of the rectum and tuberosity of the ischium, we prepare for a sight of the parts within the pelvis; this we get at by sawing through the ramus descendens pubis, and through, also, the ischium, just anterior to its spinous process; thus, we remove the whole of the tuberosity, and have before us the levator ani. Upon cleansing this muscle we mark its fibres, disposed of in a radiated direction towards the anus; it commences its origin fleshy from the spinous process of the ischium; then leaving the bone, it arises tendinous from the ileo-vesical fascia through the whole course of its curvature toward the pubis; and then fleshy again from the symphysis pubis, over the membranous part of the urethra. Its tendinous origin from the fascia is increasing the density of the latter, and it is from that circumstance, that the ileo-vesical fascia would seem of a different structure than the fascia iliaca, as before spoken of. The fibres of the levator ani, that proceed from the symphysis, are collected into a thick fleshy bundle, which runs down immediately behind the triangular ligament, and by the side of the membranous part of the urethra, to be inserted into the back part of the ligamentous process, that gives attachment to the muscles of the perineum. The next portion of the muscle being much more thin and expanded, is inserted into the extremity of the

gut, being interwoven with the fibres of the sphincter; and the fibres proceeding from the spinous process of the ischium are inserted, in common with the posterior fibres of the sphincter, into the extremity of the os coxyx. These two muscles, are nearly of a funnel-like shape, being connected together anteriorly, but allowing passage for the membranous part of the urethra, and posteriorly by the difference of the width of the sacrum. In addition to the acknowledged use of this muscle, it may be mentioned, that when the anterior part of these muscles are in action the membranous urethra must be compressed: hence they assist the acceleratores urinæ in expelling urine and semen, and would appear to have to do with spasmodic stricture, and with the other obliterations of the urethra, taking place at the membranous part. Beside these muscles, we have muscular fibres proceeding from the spinous process of the ischium and anterior edge of the posterior sacro-sciatic ligament, to be inserted in the side of the bones of the coxyx: thence called coxygeus. But from its continuity with the former muscle might, perhaps, be better understood were it termed the coxygeal portion of the levator ani.

Let us now remove the levator ani and with it the remainder of the os innominatum; in doing this, we find it necessary to detach the ileo-vesical fascia from the side of the bladder; and we have now brought to our view, a state of parts analogous to that represented by the plate. We see the triangular ligament where it adhered to the ramus pubis, and find it very thick; it is in consequence of containing the glandulæ anteprostratæ, which are situated between its laminæ; we observe, also, the prostate gland, that it is about an inch distant from the posterior surface of the triangular ligament, and that space to be occupied by the membranous part of the urethra. We do not find the anterior ligament of the bladder, in consequence of the bones having been separated at the symphysis, but we find the pouch formed by the descent of the anterior part of the ileo-vesical fascia. And we can trace the semi-elliptical line, passing from this part convex upwards, and terminating at

the extremity of the vesicula seminalis. This is the part at which the ileo-vesical fascia was detached, to give this side view of the pelvis. We observe the seminal vesicle, situated nearly an inch below this at the side of the bottom of the bladder. And we are by this show of parts, informed, that, in the adult subject, an incision of two inches in length, can be extended along the side of the bladder, beyond a lateral division of the prostate gland, in the operation of lithotomy, and in the direction ordinarily pursued for the extraction of a calculus without opening the cavity of the pelvis, as bounded inferiorly by the ileo-vesical fascia.

It now remains for me to state, that the ileo-vesical fascia sends a lamina downwards to encompass the prostate gland, the side of the bladder and rectum. This may be called the *inferior lamina* of the ileo-vesical fascia, to distinguish it from the *superior lamina*, which ascends towards the peritoneal investment formerly described. Under this the membranous part of the urethra and prostate gland, are supplied by a dense tunic, continued from the posterior surface of the triangular ligament. This affords a coat to the prostate nearly as firm as the tunica albuginea of the testis. And I have traced and lost upon this surface, many of the fibres of the muscular coat of the vesica urinaria: indeed they have appeared to me, in subjects wherein they were well marked, to converge toward the prostate from all parts of the urinary bladder.

I have now finished this anatomical sketch; and from having occupied more space than I had anticipated, I shall forbear drawing such conclusions as the statement is calculated to induce, and as I have a work nearly ready for the press, entitled, Surgical Anatomy of the Groin and Pelvis, I shall announce that, the vehicle of whatever else might be said upon this subject.

I have the honor to be, Sir,

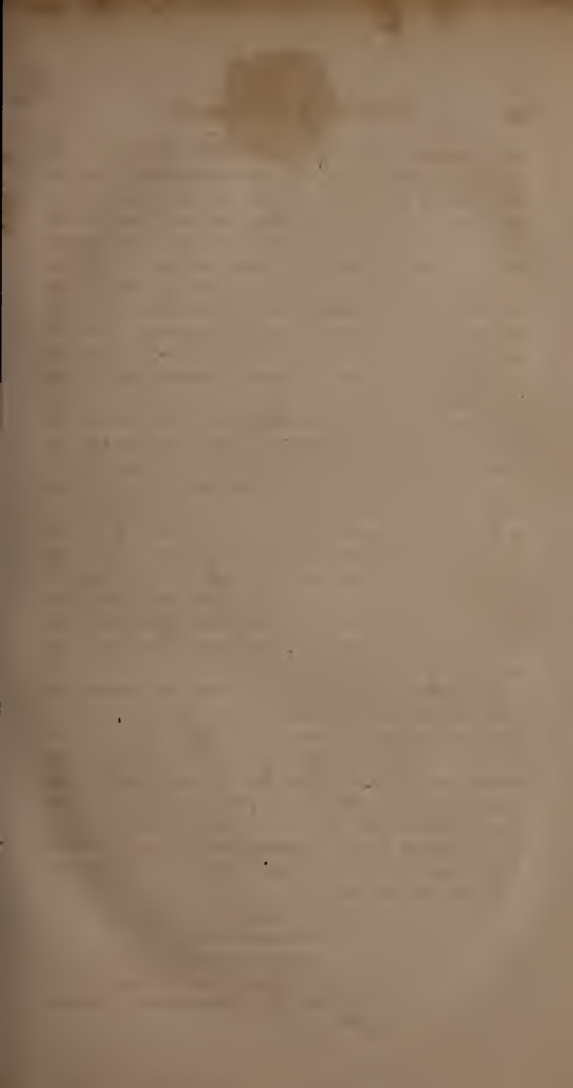
With much respect,

Your most obedient servant,

WILLIAM ANDERSON,

Licentiate of the Royal College of Surgeons, Edinburgh.

Philadelphia April 16, 1821.



P. S. *EXPLANATION OF THE PLATE.*

- A. The Urinary Bladder, distended with air.
- B. The side of the pubis entering into the articulation of the the symphysis.
- C. The articulating surface at the side of the sacrum.
- D. The spinous process of the ischium, in its relative situation at the left side of the pelvis.
- E. The rectum, partly inflated.
- F. The corpus cavernosum penis of the left side.
- G. The prostate gland.
- H. The seminal vesicle of the left side.
- a. That portion of the urinary bladder covered by the superior lamina of the ileo-vesical fascia.
- b. The suspensary ligament of the penis.
- c. The urethra, within the corpus spongiosum.
- d. The pouch at the right side, formed by the particular dipping of the ileo-vesical fascia at the anterior part of the pelvis.
- e. The line of attachment of the ileo-vesical fascia, to the side of the bladder.
- f. The bulb of the urethra.
- g. That portion of the bladder covered by the inferior lamina of the ileo-vesical fascia.
- h. The membranous part of the urethra.
- i. The crus penis detached.
- k. The triangular ligament of the urethra.
- l. The extremity of the rectum.
- m. The end of the coxyx.
- n. The termination of the ureter into the side of the bladder.
- o. That part of the bladder covered by the peritoneum.
- p. The posterior part of the line of attachment of the ileo-vesical fascia to the side of the bladder.
- q. The posterior sacro-sciatic ligament, proceeding from the side of the last bone of the sacrum, to embrace the spinous process of the ischium.
- r. The vas deferens, ascending the pelvis by the side of the bladder.
- s. The rectum, at the base of the sacrum.

ART. V. *Observations on the use of Prussic Acid in Phthisis Pulmonalis. Read before the Academy of Medicine of Philadelphia.* By JOSEPH G. NANCREDE, M. D.

THE diversity of opinions, which has lately arisen, in relation to the effects of the hydro-cyanic or prussic acid, in the treatment of phthisis pulmonalis, while its very powerful action on the human system is admitted on all sides, seem to point out this new article of the materia medica, as an interesting subject of medical investigation. Whether we consider the inquiry as having reference solely to the new remedy, which has excited so much attention; or, whether we view it as drawing anew our observations towards a disease, the inefficient treatment of which has ever been the disgrace of our science, every one of us will agree, that no subject can be more deserving our study and reflections. If on the one hand the total failure of the many attempts to discover a cure for phthisis, be calculated to destroy our ardour, in what may by some be considered a hopeless pursuit; on the other, a short insight into the baneful ravages of this fatal disease, is well calculated to arouse our attention, and urge us on to new efforts. Where is the physician, who can look at our bills of mortality, and see, without feelings of the deepest mortification, the inefficiency of medicine exemplified in the ravages of consumption? To limit our examination to the year which has just elapsed, we shall perceive that one sixth part of the mortality in the four largest cities of the United States, is the result of this disease.

Where, then, the object in view is of such immense importance, no labour will be considered useless, which can elicit any light. And whether we are destined to hail this new article as a useful addition to our means of cure, or whether we are doomed once more to witness the fallacy of our hopes, a decision grounded upon satisfactory evidence is much to be desired. For if it becomes our imperative duty to avail ourselves of every assistance which re-

cent discoveries may afford us, it is no less important to investigate by careful experiments, the properties of the substance which is proposed. This is unquestionably the only positive mode of ascertaining what value we are to give to the enthusiastic encomiums of an author, not unfrequently biassed by a love of the wonderful, and generally disposed to view his favourite remedy, in that light only which can support his particular theory. Experience must have convinced even the youngest practitioner, that this conclusion is fairly deduced from the many disappointments we incur, in relying too implicitly on the results we daily find recorded in medical works. Though it is not intended to convey the idea, that willing deception is practised, still the effect is equally prejudicial to the interests of medicine; and certainly we are justified, after numerous unsuccessful trials, in expressing some doubts as to the correctness of their results, while making all due allowance for climate, idiosyncrasy, habits, &c. It matters little to the practitioner, condemned to witness the fruitless exhibition of a medicine, extolled as a never failing specific, whether he has been misled by the enthusiasm alone of the author of it, or by his want of candour in relating every circumstance liable to affect its operations. Whoever reads with attention, the European works on medicine, and more especially those on its practice, must be astonished to perceive how widely we differ from them in the single article of doses. Nor does it appear to me easy to understand why a similar dose of medicine should produce one effect in London or Paris, and give rise to an entirely different result under similar circumstances in Philadelphia. To confine my observations to two of the most recent articles, I will mention the prussic acid, and nux vomica, both powerful agents, but, owing probably to the misrepresentation of some circumstances, having lost much, in this city at least, of the high reputation with which they were received. The former of these articles, has however been subject to an objection which may strongly affect its use here. That is, the rapidity with which it is decomposed. This difficulty is now nearly done away, as I shall state hereafter. It only remains

to ascertain by experience what are its medical properties. With the view of arriving at truth by the only safe path, and of accounting if possible for the disuse or neglect into which the first of these remedies has already fallen, I have prescribed it in a number of cases, the result of which is fairly and candidly detailed in the following pages. Of the efficacy of *nux vomica*, in several forms of diseases, I trust it will soon be in my power to publish satisfactory evidence.

Dr. Magendie, whose name is familiar to all those who have taken an interest in physiological experiments, has enjoyed until quite recently the credit of having first suggested the use of prussic acid in phthisis. Dr. Benjamin L. Oliver, a practitioner of medicine of Salem, in Massachusetts, has however asserted his claim to the priority of this idea; and in the fourth number, ninth volume of the *New England Journal of Medicine and Surgery*, he has satisfactorily proved, that as early as the year 1811, he had administered this medicine to one of his relations. Whether Dr. Oliver is entitled to the merit of this discovery or not, it is no less true that to Dr. Magendie are the profession indebted for the attention which this powerful agent has lately attracted.

The latter gentleman, for whose veracity and correctness I feel competent to vouch, from personal acquaintance, informs us, that having observed, that animals poisoned by prussic acid and entirely deprived of sensibility and motion, preserved for several hours a free respiration, and a circulation apparently uninjured, although much quickened, conceived the idea, that this substance might diminish general sensibility without injuring respiration or circulation, and therefore could perhaps be employed with advantage in those diseases arising from an increased morbid sensibility. Experience has proved, that in some cases this conclusion was a just one, and further, that although its action on the nervous system and on the blood, is similar to that of opium, yet, that it differs essentially from the latter in its sudorific qualities.

Dr. Magendie has accordingly prescribed this medicine

in a number of cases of catarrhs, recent and chronic; and also in phthisis, in the majority of them with advantage. At his request two other eminent physicians of Paris, Drs. Re-
camier and Lerminier, attached to the two first hospitals of that capital, have also administered it, and with results equally beneficial.

Dr. Granville, of London, has been no less successful in his exhibition of this medicine, and in his publication, recommends it, as a valuable remedy in pulmonary affections.

From his testimony it would appear, that no medicine hitherto used in phthisis, had afforded such permanent relief. And finally, from the united experience of Drs. Majendie and Granville it is satisfactorily established, that the prussic acid given in moderate doses, and properly diluted, has by them been advantageously employed as a palliative in the treatment of phthisis, with a view of calming the cough, facilitating expectoration, and procuring sleep; and from these properties, those gentlemen consider it entitled to the first rank among the medicines usually prescribed with these expectations. Nay, they even indulge the hope, that it will prove not only a palliative, but a radical cure, and this opinion, however chimerical it may appear to those who have attended closely to phthisis, is illustrated by several cases, which are fully detailed in the papers I allude to.

Subsequent to the publication of Dr. Granville's memoir, another on the same subject by Dr. Elliotson, of London, has made it appearance. Its perusal has afforded me a new instance of the anomalies, with which the history of medicine abounds. The author, who appears to have bestowed considerable attention on this subject, declares positively that in no case of phthisis has he ever derived *any* benefit from the acid, as a permanent remedy; and therefore that it is to be added to the list of medicines that have been fruitlessly tried to cure consumption. He admits, however, that in certain cases, it may allay the cough, and even has a tendency to diminish night sweats. Yet this concession is only made on the conditional admission, that these effects are

the result of a sympathetic action through the medium of the stomach. But he recommends its use in chronic catarrhs, nervous coughs, simple dry coughs and hooping cough. It is in cases of dyspepsia, and indeed in all the affections of the stomach accompanied with pain, that he recommends it as an invaluable remedy.

Dr. Elliotson's publication, dated July 1820, and which contains many very useful facts in relation to the effects and powers of prussic acid, is the latest European publication I have seen.

The reputed virtues of this substance could not fail to attract the attention of the profession in this country; but in no section of the United States has it met with such general encouragement as in the eastern states. The paper of Dr. Majendie has been translated by Dr. Percival, of New-Haven, and several respectable practitioners have recorded in the last number of Professor Silliman's Journal, the result of numerous trials. These have generally been favourable, and are highly calculated to encourage those who may wish to examine its medical properties.

I am far from being convinced that it has received in this city, that attention which its reputed virtues should have called forth. In fact but few of our physicians have as yet prescribed it, and those cases wherein it has been directed, are so few, as not to form any satisfactory evidence of its powers.

The high opinion I have long entertained of Dr. Majendie, and my implicit reliance on the correctness of his results, induced me soon after the reception of his paper, to undertake the repetition of his trials. It was by this method alone that I could satisfy my own mind, as to the effects of this very powerful agent, concerning which so great a discordance of opinion already existed.

CASE I. The first case which appeared to me likely to test its efficacy, was that of Mrs. H. a patient of the Philadelphia Dispensary, aged about 45 years, and who was at that time labouring under symptoms of confirmed phthisis. She had tried in vain many remedies before her ad-

mission to the care of the Institution. After resorting to the usual ineffectual modes of treatment, I had recourse to Dr. Majendie's plan, and prescribed the acid. It had been prepared with care, and was recommended to me as being precisely of the same strength as that used in Europe. Two drops were directed to be taken in pure water each day, and the dose to be gradually increased until the patient in the course of one week had reached the dose of ten drops daily. Yet no visible effect was perceptible; not only was no amelioration observed, but neither the constriction about the chest, nor head-ache, which are mentioned as frequently attending its exhibitions in such large doses, were complained of. At the end of the second week the dose had been increased to about 30 drops daily, without producing any effect whatever. I now felt discouraged, but having procured a quantity of fresh prepared acid, I had an opportunity of testing its strength. Two drops were put on the tongue of a kitten two weeks old; it was immediately seized with convulsions, and in about four seconds expired. The patient now began its use in doses of two drops daily for three days, then increasing in the ratio of three drops daily, until she took 30 drops in the twenty-four hours. At this period, I could find no visible alteration, and with reluctance abandoned, in this case, the use of the medicine.

The instance of Mrs. H., who was an extremely irritable and nervous woman, and whose disease was complicated with asthma, could not be deemed a sufficient trial, even admitting it to be a fair one, and consequently was in no ways likely to affect the question of the efficacy of the medicine. I soon found, among the patients of the Dispensary, a case which afforded some rational hope of success.

CASE II. John C., aged about fifty-three years, of a strong and muscular appearance, working in a brick yard, had been troubled for about two months in August 1819, with continual pain in the chest, dyspnœa, and cough, accompanied with some expectoration of a doubtful nature. The pain in the chest induced me to direct the loss of about fourteen ounces of blood, and immediately after he began the use

of the acid in daily doses of four drops, increasing two drops every day until he had taken twenty-four in a day. He now informed me that his cough was somewhat better, and that his sleep, which had been very much disturbed by unpleasant dreams, and otherwise agitated, was more tranquil. The dose was continued thus for a few days, and then increased four drops daily, until the patient took daily thirty-six drops. The benefit derived had increased, his cough was better, and his sleep continued composed. The medicine was now at an end, and this case terminated here. It is, however, proper to mention that I have seen him a month ago, still affected with slight cough, but which is considerably diminished. He is now enabled to pursue his work. I conclude, therefore, that in this case, the prussic acid has been of some use.

CASE III. In November last, Robert N., a patient of the Dispensary, aged about thirty-six years, of a slender form, and by trade a carpenter, applied for medical assistance. He complained of pain in the chest, great difficulty in breathing, or, as he expressed it, a shortness of breath, a dry cough, no expectoration, and occasional chills and night sweats. His pulse was tense and frequent. These symptoms had been gradually increasing for about six months, during which time the patient had lost considerable flesh. After taking for a few nights the ipecacuanha and opium combined, he began the use of the acid. At first he took it in very small doses, owing to the impression I was then under that its effects would be very powerful. But the second prescription, November 28th, contained thirty drops in four ounces of water, edulcorated with gum arabic and sugar. The patient was directed to take a table spoonful of the mixture three times each day, and he soon felt some amelioration in his feelings.

December 2d, the mixture being out, a new one, containing ʒij prussic acid in the same vehicle, and accompanied with the same injunctions as to its doses, was directed. It was followed by increased benefit.

December 7th, one drachm of the acid in three ounces of

water, edulcorated with gum arabic and sugar, which the patient is to take in table spoonfuls every third hour. Robert states that his cough is considerably diminished.

December 13th, the same prescription is renewed with manifest advantage.

December 18th, one drachm and a half in six ounces of water is prescribed to be used in the same doses and at the same intervals. The patient continues improving. At no period since its first exhibition has he mentioned either nausea or head-ache.

Robert continued under the operation of the acid until the 20th of January, and then used as a substitute and general tonic, the cold infusion of bark. His cough has nearly left him, and his appetite, appearance, and general feelings are about the same as previous to the invasion of the disease. He is discharged from the care of the institution, and is now pursuing his trade.

CASE IV. Mary P., aged about twenty-two years, had been affected for about three weeks with the usual symptoms of incipient phthisis, and pronounced to be affected with that disease by a respectable physician who had visited her previous to my attendance. When I first called on her on the 11th of February, I found the patient confined to her bed with continual fever, and a small, quick, and tense pulse. She had already used in vain several pectoral mixtures, and took, according to my directions, a fresh one containing balsam tolu; but without deriving from either any relief whatever. Accordingly, very soon after my attendance commenced, the prussic acid was administered in doses of one drop to the ounce of water, which doses were very soon increased. She is now taking it in doses of sixteen drops to four ounces of water, a table spoonful every two hours. Without exhausting your patience, gentlemen, with details which, by their repetitions, may be tedious and uninteresting, I will merely state that my patient is rapidly improving, so much so that this day, the 11th of March, she informs me that she has not coughed more than twice or three times in the course of the last twenty-four hours. She is now sitting up,

and has already acquired some flesh, and a slow and rational pulse.

Besides the cases already related, there are three others under care, but they are of too recent date to form at present any satisfactory conclusion. A suspension or a mitigation of some of the symptoms of a disease for a few hours, or even days, should never be admitted as positive testimony. It becomes proper, however, to state, that the patients are all under the impression that they derive benefit from this medicine.

It may, perhaps, be expected that in concluding these remarks, something should be offered tending to explain the *modus operandi* of the article under consideration. But besides the want of sufficient experience to support the reputation which this new medicine has been supposed to deserve, no facts explaining its action, of a satisfactory nature, have as yet been offered by those more immediately concerned in its success. It has been compared to opium in some points of view, and it does appear to possess some sedative properties. That it acts in some cases by occasioning vertigo, headache, and a slight stricture across the chest, must be admitted; but these are merely temporary effects and of very rare occurrence. In my limited circle of observation, I have not witnessed the alarming symptoms related by Dr. Majendie, nor have I so far been enabled to produce those very agreeable sensations which Dr. Granville informs us were visible in some of his patients.

Diffident, however, of my own results, and therefore particularly anxious that some of my colleagues should add confirmation to the facts herein stated, I have thought the following remarks not altogether uninteresting to those who may feel disposed to use this medicine.

There are several modes of preparing the prussic acid, three of which are recommended in the French codex, under the names of Scheele, Gay Lussac, and Vauquelin. The two latter are much stronger than the former, but all three lose their properties by exposure to light and air in a very

short time. To obviate this very serious objection, Dr. Cooper has prepared a solution of this acid in alcohol, and in this form it certainly is much more easily preserved than the other preparations. It would appear, however, that it is still subject to a slow decomposition, from the circumstance of its peculiar flavour, resembling that of bitter almonds, decreasing after being kept for a couple or three weeks, and therefore it becomes desirable that it should be renewed as often as possible.

I have used this last preparation in most of the cases referred to in these remarks. In no case has its bad effects been produced, although used in large doses. For instance, I am in the habit of prescribing it in the proportion of one drop of the acid to an ounce of water, a table spoonful of the mixture to be repeated once, twice, or oftener, as it may be judged requisite in the course of the day. In the case of Daniel N. a mixture containing one drachm and a half of the acid in six ounces of water, was directed to be taken by table spoonfuls every third hour, and the patient was evidently benefitted by its use. Yet this is but one case, and should not be considered as conclusive evidence in favour of such large doses. For my very worthy and experienced friend Dr. Monges, of this city, informs me that he prescribed this same article in a case of calculous phthisis, a short time since, and that doses of one sixteenth part of a drop, occasioned such an increase of the cough, and consequent irritation, as to induce him to abandon its use. He observed at the same time, that in this instance the particular circumstance of small portions of calculi being expectorated, perhaps in a more rapid manner in consequence of the exhibition of this medicine, may have prevented its beneficial effects.

One inference appears fairly deduced from the above facts, and that is, that we should receive with due caution the opinion contained in Dr. Elliotson's paper,* when he affirms that he does not hesitate to prescribe the acid for patients whom he has not an opportunity of seeing more than once

* Elliotson on *Prussic Acid*, p. 40.

a week, or who perhaps reside in the country. With an agent endowed with such powerful properties, it certainly would seem to require more circumspection, than can be exercised under such circumstances.

If any additional motive could be required, to warn the profession from too hasty an adoption of either of the opposite conclusions contained in the papers referred to, without further experience on so interesting a subject, it will be found in a reference to the history of other medicines, which have had their day, and which have also been proposed with the hope of averting this scourge of the human race. Many of these, apparently gifted with powerful properties, and supported by the enthusiastic encomiums of highly respectable names, have nevertheless not been found capable of withstanding the test of practical experience, and have accordingly already been deprived of their short-lived reputation and ill-deserved fame.

ART. VI. *Strictures upon Dr. Merriman's Opinions of Retroversion of the Uterus and Extra-Uterine Conception.* By WILLIAM P. DEWEES, M. D.

THE observations now presented, form a sequel to those offered on "Retroversion of the Uterus," in No. 2, of the Philadelphia Journal of Medical and Physical Sciences, and originally constituted a part of them. I was induced to divide the paper from its great length to which it had unexpectedly run, and for which I have since been rejoiced, as I have been afforded the opportunity of reading Dr. King's "Analysis on the subject of Extra-uterine gestation and retroversion of the gravid uterus." Dr. King, in several instances, employs the same arguments against Dr. Merriman as I have used; I shall therefore when this occurs, avail myself of his authority to support my own; and I shall do this with the more confidence, as Dr. K. agrees with Dr. M. in the belief of the possibility of the uterus remain-

ing in a state of retroversion until the full period of utero-gestation, [though decidedly hostile to many of his "proofs;"] while I cannot for a moment admit it. As my object in this paper is to illicit truth, I shall in conducting it to issue, give every consideration to the arguments of Dr. M. they appear to merit; and where I differ from him, as I am obliged almost *toto calo* to do, I trust I shall conduct my arguments with that liberality and temper that should ever characterize discussions of this kind.

Medical science has sustained great injury from overweening zeal, and the inordinate desire of generalizing; cases, chancing to depart from their ordinary course, have with too much facility given rise to hypotheses to which the best interests of humanity have been sacrificed; and certain vague phenomena, have laid the foundation for practices at variance both with experience and common sense. These positions, we think, are well illustrated by the principles which govern certain individuals in the treatment of the diseases we are considering; for instance, in the retroversion of the uterus, because, in a few instances spontaneous reposition has taken place in the early months of pregnancy, it has been falsely concluded that the interference of art was seldom or never necessary; and from two to three cases of an anomalous kind, but supposed to be retroversion at full time, and which, after long continued and terribly severe sufferings, did well, it is attempted to be established that the uterus can remain retroverted until this period, and the woman be relieved by the efforts of nature alone; and as an inference from these very doubtful cases, it is declared, that the instances purporting to be cases of extra-uterine fœtation are not such, but are truly evidences of retroversion at the full period of utero-gestation, but where the uterus had suffered lesion either from rupture or ulceration, and thus permitted the fœtus to escape into the cavity of the uterus, or to place itself between the vagina and rectum.

In matters purely speculative, little injury, perhaps, is done by giving rein to the imagination, or forcing facts to

square with a favourite hypothesis; but in practical matters too much care cannot be taken to guard against the vagaries of fancy, or the misapplication of facts. This in an especial manner should be avoided by those who give tone to public opinion, or can direct the public judgment. In this particular we fear that Dr. M. is somewhat reprehensible, as he appears to enjoy a reputation for talent and experience that would justify, perhaps, any one who might adopt his opinions. He appears to us to have admitted with too little hesitation or scrutiny, circumstances that would, to say the least, admit of very different explanation, and deduced from them conclusions at variance with many well established facts. It is our design in this paper to redeem the pledge that these assertions would seem to impose upon us; in prosecuting which we shall first consider the facts upon which Dr. M. has built his hypothesis, by commenting upon them in detail, either as related by himself, or referring to those authorities wherever there may be a material omission on the part of Dr. M. in the history he gives of them; second, attempt explanations adverse to those of Dr. M. from the facts employed by him, or citing those which have occurred in our reading or observation.

Dr. M. commences, or rather introduces his subject by the following observations.* “The records of medicine are abundantly supplied with reports of cases of extra-uterine gestation. These we may divide into two classes:—First, where the conception has been detained and nourished in some of the appendages of the uterus and the uterine system. Secondly, where the nourishment and maturation of the fœtus have been supposed to be effected in a receptacle separate and apart from the uterus and uterine system. This last opinion seems to have been adopted too readily; at least it will, I am fully persuaded, be found, upon a nice and accurate examination of this latter class, that the writers of the cases were not so minutely exact in their ob-

* Preface, p. v.

servations, nor so explicit in the detail of many very important and essential particulars, as to stamp with credulity an occurrence so incomprehensible and so repugnant to the usual operations of nature." The question may well be asked, whether every deviation from the natural order of things can be considered any thing more than an exception? It does not necessarily imply an impossibility, nor can we believe all have been mistaken who have related cases purporting to be of extra-uterine conceptions, or that they have "omitted important and essential particulars" to make their statements clear as to the points they wished to establish. What is there more extraordinary in a ventral conception than in fœtuses being found in the abdomen of both boys and virgins? * Besides, the relators of these histories had no theories to support, and are consequently to be supposed free from all bias upon the subject. They may be believed to have faithfully detailed what they saw, without a motive to disguise or mislead. If this be so, we cannot hesitate, without being chargeable with undue scepticism, to believe them to be unequivocally cases of extra-uterine conception. But if it could really be proved that there were occasional errors upon this subject, it is nevertheless certain that there have been indisputable instances of extra-uterine pregnancies; and this is sufficient to destroy the conclusion that these occurrences are impossible, because to Dr. M. they are "incomprehensible and repugnant to the usual operations of nature."

Dr. M. has espoused the notions of Dr. Denman upon the subject of retroversion; and it is important to the investigation we are about to make, that these opinions be briefly noticed, that the subsequent observations may be the better understood. In his theory of its formation, and his directions for its relief, he but repeats that celebrated accoucheur. We shall therefore not follow him in detail, as what we have said in our former paper, will apply as strictly to him, as to Dr. D. We cannot however pass in silence

* Duncan's Annals of Medicine. Medico Chirur. Trans. vol. i. p. 136. Ibid. vol. vi. p. 124. Med. Repos. vol. xiii. p. 1

the following curious directions for the relief of this complaint. "Respecting the method of curing retroversions of the womb, enough has been said to show that the principal reliance is to be placed upon the introduction of the catheter; this should be done twice at least or oftener in the twenty-four hours. Care is likewise to be taken to keep the bowels open, and rest is to be enjoined. By pursuing this plan steadily, the mal-position of the uterus is usually overcome in a few days. It is still customary with some practitioners of eminence, to make use of artificial means for replacing the womb, after the bladder has been emptied, and the bowels opened; and there can be no great objection to make such attempt, provided it be done cautiously, and that no force be made use of. *In general however, nothing of this kind is either necessary or adviseable.*"

Now, it is admitted by all who have written on this disease, that there is often a difficulty and sometimes an *impossibility* to introduce the catheter; and Dr. Denman* informs us, that "many women have lost their lives from the want of expertness in introducing it." "Expertness" can only be acquired by repeated opportunities; yet a young practitioner, in many instances, must be the operator. If this be so, the woman agreeably to the directions of Dr. M. is exposed to greater risk from the mal-adroitness of her operator in the introduction of the catheter, than from the "force" which he so earnestly deprecates. Dr. Denman also acknowledges, "that there is an obstinate constipation of the bowels, produced by the pressure of the uterus upon the rectum, which renders the injection of a glyster very difficult, or *even impossible*;"† and we know that in most of these cases, purgatives are totally unavailing—yet we are to draw off the water two or three times a day, and to keep the bowels open!

Dr. M. has no objection to reposition, "provided it be done cautiously," and without force. Now, force being a relative term, we cannot with certainty determine that exact degree of it, which would in the opinion of Dr. M. be

* Introduction, vol. i. p. 141.

† Introduction, vol. i. p. 137.

objectionable. If the operation be attempted by a man well instructed in his profession,* no more "force" we trust will be used than is necessary to the end; and if this be well directed, upon a uterus that is capable of reduction, it will not be so great as to do mischief; and if less be employed, the end cannot be ensured. Besides, it is well known that this operation has been very frequently performed, without any injury resulting from it. From this it is evident, that Dr. M. adopts in the fullest extent, the opinions of Dr. Denman, upon the causes, mechanism and mode of treatment of the retroversion of the uterus; and we hope to make it equally clear, that it was this affiliation, that led Dr. M. to the unsatisfactory explanations with which his hypotheses abound.

But let us recur to the main object of these "strictures." Dr. M. endeavours to prove by the history of several cases, "that the uterus which has become retroverted in the earlier months of pregnancy, may continue in that state till the full period of gestation has elapsed." And says, "whenever a retroversion of the uterus shall have taken place, and have continued to the end of utero-gestation, it may be expected that the case would terminate in one of three ways:"

"First,—The good form of the pelvis, the health and strength of the mother, and the efficacy and continuance of the pains, may all combine to replace the uterus and produce a favourable issue." In conducting our remarks we shall consider the cases he offers as proofs of this opinion in the order Dr. M. presents them. He commences by admitting, with every writer almost upon this subject, that very serious consequences may result from the untoward situation of the uterus, called "Retroversion," but adds, "it is, however consolatory to know, that under some circumstances, the uterus may remain in a state of

* The most simple and the most necessary operation, in the hands of an ill-instructed and rude practitioner, may be followed by the most serious consequences—of the truth of this, the history of our profession affords but too many examples—and should an operation be proscribed, because perchance, it may be ill conducted, we should have but few, in the wide range of human misery, that could be ventured on.

retroversion for a long time, even to the completion of the period of utero-gestation, without producing a total suppression of urine or any other uncommon or alarming symptom." He attempts to support this postulate by the relation of several remarkable cases, almost all of which are recorded *by their authors* as instances of extra-uterine conceptions. He observes that the first case of this kind that was ever published in *proof* of such a fact, is recorded by Dr. Henry Seguin Jackson in a useful little work, entitled "Cautions to Women, &c." We referred to this work with the hope of finding in it a detailed statement of this case, and the reasons which induced Dr. Jackson to consider it an instance "of a woman reaching the full period of gestation with a retroverted uterus," but in this we were disappointed—for Dr. J. barely asserts "that he had an opportunity of seeing such a case, in company with Drs. Bland, Denman, Thynne, Merriman, and Croft;" and that "the situation of the woman at first appeared inexplicable, and she continued several days in labour, but the gradual efforts of *nature* at length, completed her delivery, by restoring the womb *nearly* to its natural situation."

From this vague, and unsatisfactory account, Dr. M. concludes this to be a case of retroversion continuing until the last period of utero-gestation. It may have been sufficient for him, who perhaps from personal knowledge of Dr. Jackson could rely upon his accuracy; but more is required, by those who have not this advantage, especially as all *à priori* reasoning would consider this condition, a physical impossibility. Dr. M. has we believe too hastily decided upon this case; and the more especially if he was in possession of no more of the circumstances than have been related by Dr. Jackson;* it is true indeed that he again advert to this case, which would lead one to the belief he was in possession of more than he makes meet the eye; but he neither details the case, nor indicates the authority for saying that this patient "never had an entire suppression of urine, but suffered severely from partial suppression and

dysury, between the third and fourth months of her pregnancy." We would ask any candid man conversant in obstetrical practice, whether a "suppression of urine and dysury," characterise with sufficient accuracy a retroversion of the uterus. Surely they do not; yet these symptoms are the only evidences offered the public of this being a case of retroversion of the uterus.*

We here may again take occasion to lament, "that if a woman about the third month of her pregnancy, has a suppression of urine continuing for a certain time and producing a certain degree of distention of the bladder, we may be assured that the uterus is retroverted," should be considered as the diagnostics of this complaint; it has evidently betrayed Dr. M. into a very questionable conclusion, and shaken our confidence in both his candour, and the accuracy of his observations. We hesitate the more to admit this to be a case of retroversion protracted to the "full period of utero-gestation," as he immediately afterwards informs us that "as she advanced in her pregnancy, she was relieved from much of this inconvenience, *probably* by the parts adapting themselves to the situation she was in." Is it within the scope of common belief, that a retroverted uterus at the full period of gestation, can occasion less inconvenience than at the third or fourth month of pregnancy? Do not the histories of fatal cases declare exactly the contrary? The one related by Dr. Hunter† demonstrates, that even at the fourth month, the pelvic cavity can be so entirely occupied by the uterus, as not only to produce a suppression of urine, but to require great force, and a division of the symphysis of the ossa pubis, before it could be liberated from its confined position—can it then be for a moment believed, that a fully distended uterus, in a state of retroversion, would create fewer evils than one at the fourth month? We could easily multiply examples of this kind; but we are restricted by the fear, that this paper would run to too great a length. From what we have said, we think that Dr. M. is by no means entitled to the conclusion, that

* P. 27. † Med. Obser. vol. iv. p. 400.

this was "a case of retroversion, at the full time of utero-gestation;" for we are fully persuaded, that so much of Mrs. Wilkes's case as has been detailed by Dr. M. can be explained upon very different principles.

The next case related by Dr. M. in support of his opinion, fell under his own observation; and if carelessly read, might be considered as a proof of his position; but if strictly inquired into, will be found as equivocal as the one related by Dr. Jackson. Dr. M. informs us, that "Mrs. F**** became pregnant for the first time, about September 1805. She did not suffer more during her pregnancy than most other women, except that for the *last two or three months, she was troubled with difficulty of parting with her urine, and considerable pain in the act of passing it; yet her sufferings in this particular, were not so great as to induce her to consult her accoucheur upon the subject.* She neither at this time, nor at an earlier period of her pregnancy, experienced a total suppression of urine, nor does she recollect having ever retained it long enough to occasion any considerable inconvenience. When about five months advanced in her pregnancy, she was much terrified and afflicted on hearing of the sudden death of an aunt; which, as she herself expressed it, seemed to turn her whole inside upside down; and to this she imputes that alteration in the situation of the womb, which *she was given to understand either then or at some other time took place.*"

This case is remarkable for several of its particulars.—First, It is supposed that the retroversion took place at the fifth month of her pregnancy; yet we are informed she never suffered any remarkable difficulty in passing her water, nor ever had a "total suppression;" and indeed the symptoms did not take place until the "last two or three months of her pregnancy." We would inquire what evidence there is, that the retroversion, if it really existed, took place at this period of gestation? there is none—the woman's declaration, that the tidings of her aunt's sudden death "seemed to turn her whole inside upside down," does not prove it produced retroversion of the womb. Besides, this is a familiar mode

of expression with women when suddenly alarmed, and really means with them nothing more than that flurry of spirits and hurry of circulation that is invariably consequent upon surprise; it is almost always accompanied with palpitation of the heart, and great commotion about the pericordia, and hence the expression. Second, This "turning upside down," was not followed by one symptom that we are informed of, that would lead to the suspicion of a retroverted uterus; the patient experienced no inconvenience from her urine until the two or three last months of her pregnancy, and then it was not so intense as "to induce her to consult her accoucheur." Third, The period at which this accident is supposed to have taken place, makes it liable to strong suspicion; for it seems admitted, that the bulk of the uterus at the fifth month* is too great to permit this change to take place, unless there be some accompanying circumstance which may prevent the uterus from developing itself to the size which is usual at this period of utero-gestation—now, we are not apprised of any such circumstance, but are told on the contrary, that Mrs. F. "did not suffer more during her pregnancy, than most other women." Can we then for a moment imagine, that the uterus became retroverted at the time supposed by Dr. M., since had it happened at that period of gestation, the size of the uterus must have been such, as to have filled the pelvic cavity, and at once given evidence that this change had really taken place? or if it took place, she could not be so far advanced in her pregnancy as is supposed. Fourth, The patient herself attached at the moment no consequence to this alarm; for it was not until she was informed that a change had taken place in the situation of the uterus, that she adverted to it; "it then seemed to her the only mode of accounting for it."

Mrs. F. was taken in labour in June, which from the period it is stated she became pregnant, would make the full

* "After the fourth month, its volume (the uterus) is generally so great that it cannot suffer such a deplacement; because its height at that time, in most women, exceeds the breadth of the pelvis, taken from pubis to sacrum."

Baudelocque, System of Midwifery, Vol. 1, p. 163.

time of gestation.—She had considerable pain—no os uteri could be found, and the whole examination per vaginam, led to the belief of retroversion.—On the second day of her labour she was attacked with fever, delirium and convulsions—these all subsided by a bleeding. After a continuance of more or less pain for five days, there was “a mucous discharge tinged with blood” perceived, upon withdrawing “the finger from the ossa pubis,” which led to the conclusion “that the os uteri was situated in that direction.” “On the sixth day of labour a considerable alteration was discovered in the pains, and in the situation of the hard semi-globular substance which occupied the pelvis.” On examination, there was found between the ossa pubis and “body of the uterus,” a thick fleshy substance of a flattened form, which descended into the vagina, while the tumour began to recede; “in a few hours more the womb was restored to its proper situation, and the os uteri could now be distinctly felt coming down from a considerable height above the pubes, and the child’s head behind it.”

“As the flattened substance just mentioned came more within reach, it was perceptibly distended by some fluid during every pain, and as it approached the os externum, resembled, to the touch, the membranes distended with the liquor amnii, but those membranes much thicker than in the natural state. Shortly after some of the bones of the cranium became loosened, and were forced down into this bag or pouch; and now it became evident that this was only the scalp of a dead and putrid child, and that its distension was owing to the contents of the cranium being forced into it by the pressure of the pains.”

“The death of the child being thus clearly ascertained, Dr. Merriman (the uncle of the author) determined upon making an opening through the distended scalp, in order to evacuate its contents, which he received in a basin, *nearly to the amount of a pint. It consisted entirely of grumous blood and the brains of the putrid fœtus.*”

This case may be looked upon as a case of retroversion terminating in *abortion*; for we cannot agree with

Dr. M. that it is an instance at full period, as he has not, in our opinion substantiated it, either by the detail of the case or his explanations of it. Our reasons for dissent are, First, That no symptoms which would characterize this displacement, or even lead to a suspicion of it, existed until "the two or three last months of pregnancy," and they were not so severe as is usual with this complaint. Second, That we have no instance upon record where this disease was not accompanied with distressing symptoms, unless it be, perhaps, in the very early months. Third, That the symptoms which usually attend retroversion did not take place until (agreeably to the reckoning Dr. M. gives us) the sixth or seventh month of utero-gestation; a period at which Dr. M. himself would declare it would be impossible it should happen, unless some attending circumstance in the woman's health should prevent the uterus developing itself agreeably to the usual order of this process; but we are not informed of any such condition; but, on the contrary, it is expressly said, that "Mrs. F. did not suffer more than women usually do." Fourth, That, were this a case of retroversion at full time, the pelvis should have been completely occupied by the enlarged uterus; but this was not the case, for there was a free issue to the fæces, and, for the most part, to the urine.— Could this possibly be the case if the uterus were fully developed? At the third month in many instances, the suppression of urine, and the interruption to the discharge of the fæces, is complete. This happened in the case related by Dr. Bell.* It obtained in many others we could relate; it was an attendant at the fourth month in Dr. Hunter's case,† besides others. Can it then be, for a moment, admitted, that neither of these circumstances should exist when the uterus had acquired three times the size as at these periods? Fifth, That it amounts almost to a demonstration that Mrs. F. could not be much beyond the fifth month of pregnancy, when we consider the size or capacity

* Medical Facts, vol. viii. p. 32.

† Med. Observ. vol. iv. p. 400.

of the child's cranium, which is determined by its contents, "measuring nearly a pint," and consisting "entirely of grumous blood and brains."

Now let us suppose that one half of the fluid discharged by opening the scalp was brain, would the space occupied by less than an half pint be equal to that of a head at full time? Certainly not. There is another circumstance related in the history of this case, which, in our view, strongly corroborates the opinion we have just ventured to advance, (namely, that Mrs. F. was not in her ninth month of pregnancy at the time of her labour,) which is this: that "by applying the hand upon the abdomen, there could be very distinctly felt two tumours in the left hypogastric region." The uterus at full period, it is well known, would have reached considerably above the navel, and would remain there until the expulsion of the head from the vagina should oblige it to descend; and even at this moment it would scarcely be so low as the umbilicus. The two tumours which were "distinctly felt," were most probably diseased ovaria, and thrown over to the left side in consequence of the displacement of the uterus; and this conjecture is rendered more than probable from the following considerations:—First, That were it an uterus at full time, though retroverted, it could not occupy the "left hypochondriac region exclusively. This must be obvious to every one. Second, That these tumours were "felt to be lying so very near the surface," says Dr. M. "that we could hardly conceive *that the parietes of even a remarkably thin uterus* could be interposed between the integuments and the child." Third, Because it is immediately after stated, that "from feeling the fundus uteri through the rectum, we were led to believe, that the parietes of the uterus were of the usual thickness." Fourth, Because Mrs. F., Dr. M. tells us, "has not been pregnant since," making a period of five years* since her delivery. This last reason may not perhaps appear as conclusive as those urged above, since

* Dr Merriman's Essay, we are informed, was first published in 1810.

the uterus itself may have been injured, and thus prevent impregnation. This may be so; but Dr. M. tells us "that no opportunity had offered to ascertain the exact state of the uterus; but there is no reason to suppose that it sustained any injury from the awkward situation into which it was thrown."

The next authority Dr. M. produces in support of his opinion, is Deventer—in making use of him we are sorry to say, he has made garbled extracts; and has forced his meanings. Dr. M. has used just as much of his author as suited his views, whereas, had he given Deventer fairly, he would be decidedly against him. We shall therefore give extracts from this judicious author to prove what we have just advanced; and we do this the more willingly, as his work is in the hands of very few in this country. In making use of Deventer's description "of a certain kind of Labour," all he says upon this subject, must be admitted to be, either correct, or incorrect; if the first, we shall prove Dr. M. in the wrong; if the second, he must not be considered as authority, and consequently Dr. M. will have no right to avail himself of his errors. Deventer's general accuracy is admitted by all; and Dr. M. believes "his integrity has never been called in question"—what he relates then, must be received as the result of experience; as he himself assures us it is; he says, "I have found from experience that the womb may be too much resupinated, or incline with its fundus backwards, being forced against the back bone, so that its mouth or passage is not only raised too high into the abdomen, but is so obliquely situated, that its axis no longer corresponds with that of the vagina."

This account of the malposition of the uterus may be considered as a brief description of what Deventer considered a posterior obliquity of this viscus; and which Dr. M. endeavours to show is a retroversion at full period. But we cannot find in the entire chapter upon this subject, any thing to confirm this opinion—Deventer is very particular in his history of this supposed obliquity; but there is no mention of a single symptom that would lead to the belief, that it is

a retroversion he is describing under a wrong name.* Dr. M. confesses "it is remarkable, that the authors who followed Deventer upon the subject of the obliquities of the uterus, *and who copy his description of this position, do not once speak of it as having occurred in their own practice or knowledge.*" It is indeed remarkable, that such silence should have been observed; and we think this silence most decidedly proves, that the retroversion at full period, (for such is Deventer's posterior obliquity, agreeably to Dr. M.) is of most rare occurrence, since neither Mauriceau, La Motte, Levret, Baudelocque, Smellie, Hunter, Denman, Sa-combe, Spence, Hamilton, Gardien, Maygrier, &c. &c. give us any account of it.

We will however make a few extracts from Deventer to prove Dr. M. has garbled his account of the displacement he intended to describe—we shall quote Dr. M. so far as he goes, and supply such omissions as we think important, and place them in *italics* that they may be distinguished and compared with what Dr. M. has quoted to serve his purpose. Deventer says—"it sometimes happens that the mid-wife cannot touch the os uteri, or at least only a small edge of it, and that only when the os uteri is widely open," *for the child's head fixes it upon the os pubis, therefore the whole margin cannot be touched, since the finger can only come in contact with the lower part.* In this quotation Dr. M. has omitted a part of the text that is very important—for Deventer expressly says, that the child's head is pressed upon the os pubis; now, this could not possibly be the case were it a retroversion, since in this case the fundus must be downwards, and the head free above the superior strait, and entirely out of the reach of the finger; besides, one portion of the edge of the opened os uteri can be felt at

* Chapman it would seem had met with cases similar to those described by Deventer; for in giving the history of three women who had suffered from lacerated vaginæ, he observes, "I have been called to three women where, upon searching, I found the vaginæ quite broken through in the back part, by the mid-wife's rudely thrusting up her hand, in order to come at the opening of the womb, which in all these subjects happened to lie very forward towards, and indeed in some measure, under the os pubis."—*Midwifery*, case xxx. p. 125.

the lower part—now in Dr. M.'s case above noticed which we believe was a retroversion, he declares that the os uteri could not be felt. "In order to touch it then," says Deventer, "she must pass up her fingers cautiously, between the neck of the bladder and the mouth of the womb; for if she introduces her fingers backwards towards the rectum, she will find nothing but a kind of bag or pouch, into which there is no opening; if she presses a little forcibly against this, she may ignorantly conclude, she is feeling the head of the child, not considering that it is still enveloped by the uterus, and that she would in vain expect its descent. In this state of things the skilful midwife may discover, near the neck of the bladder, something like a semi-circular margin; this is the edge of the os uteri—here *if her fingers can reach high enough*, the midwife may even feel the hard globular part of the child's head, and the soft part, or aperture of the vertex; whence it may be collected, that the body of the child, together with the uterus, is pressed too much against the back bone."

We were not a little surprised that Dr. M. should have employed the latter part of this description, since it alone, without any of the context, is sufficient to destroy his hypothesis. Let this be compared with Dr. M.'s account of Mrs. F.'s case, and it will be seen that they do not agree in any one particular. In Mrs. F.'s case, "the os uteri could not be felt," owing to the vagina being wholly occupied by "a large hard semi-globular substance." In Deventer's account the os uteri can be felt, and no mention is made of any substance filling up the vagina. In Mrs. F.'s case the finger might be passed between the hard substance which was in the vagina and the ossa pubis, as high as "it could reach, without discovering any traces of the os uteri." In Deventer's the head of the child was "fixed upon the os pubis," and a portion of the circle of the mouth of the uterus could be touched. In Mrs. F.'s case the nature of the presentation could not be ascertained, as the situation of the os uteri was unknown to Dr. M.; for he was obliged to "some mucus tinged with blood" for the belief

that it was situated behind the ossa pubis," as "it was impossible, by the most particular and careful examination, to discover the os uteri by the touch." Besides, in Mrs. F.'s case "the semi-globular substance was, by the force of pain, made to advance near to the perineum," and could, by introducing a finger into the rectum, be "more distinctly traced." In Deventer's account we are distinctly told, that if the finger be carried towards the rectum, it would find nothing but a little close bag; and if pretty strongly pressed, the child's head could be detected through the parietes of the womb, and which the midwife would in vain wait for its descent. In Mrs F.'s case the idea conveyed by touching per anum "was, that the fundus uteri, containing either the head or nates of a child, had fallen down between the vagina and rectum; thus filling up almost the whole space of the pelvis." In Deventer nothing analogous is to be found; but, on the contrary, he tells us, and which Dr. M. takes care to suppress, "that from the situation of the uterus above described, the head, though never so well presenting, falls upon the ossa pubis," against which, by the force of pains, it is violently pressed, where its soft head sticking, is in some measure crushed, so that, *not being able to fall into the cavity of the pelvis*, the mother can by no means bring forth the infant, unless the head be removed and brought into the cavity of the pelvis.

In the whole of Dr. M.'s history of Mrs. F.'s case, it is evident that neither the os uteri nor the presenting part could be detected: yet Deventer tells us that the head, or even the fontanelle of the vertex can be felt. Dr. M. has made this part of Deventer very obscure by his translation, by affixing a condition not intended by his author. He makes Deventer say, "Here (the edge of the mouth of the uterus) *if her fingers can reach high enough*, the midwife may even feel the hard globular part of the child's head, and the soft part, or aperture of the vertex;" whereas Deventer merely states "that if she (the midwife) penetrates to the edge of the uterus, then the head, &c. may be felt!" It was important to Dr. M.'s hypothesis that the possibility

of carrying the finger sufficiently high to touch the head should be called in question, since it was impossible for him, in Mrs. F.'s case, to do so: yet it is evident, that, could he have reached *far enough* behind the ossa pubis, he must have encountered the os uteri. It was therefore of consequence to reduce Deventer's case to the same condition with his own.

It is moreover certain, that Deventer meant the body and legs of the child were up in the abdomen; for he says, that from this (the account just given of the situation of the child's head), we may be certain that the infant as well as the womb are too strongly pressed against the dorsal spine. Now this could not be, if these parts were not above the superior strait of the pelvis—but to put this matter beyond all dispute, let us refer to his 34th, 35th, and 36th plates; in all of which, the trunk and inferior extremities are represented as at the fundus of the uterus, and that made to rest against the lumbar vertebræ. It is only surprising that Dr. M. with these plates before his eyes should have attempted to force Deventer's opinions to quadrate with his own. We might add to this, that Deventer's directions for the remedying of this malposition, confirm what has just been advanced—they are too long for insertion, and we beg therefore merely to refer to them.

Dr. M. says, "these extracts" (namely those quoted from him), "tend to show, that Deventer was describing a wrong position of the womb, under the denomination of an obliquity of the uterus, which was in reality a retroversion of that organ; for, how could the passage of the finger towards the os sacrum, be prevented by a substance in the vagina, feeling like the head of the child, while the os uteri was almost out of reach above the ossa pubis, but by a retroversion of the uterus?" To this we answer, that Deventer makes no mention of any substance in the vagina which would obstruct the passage of the finger towards the os sacrum; on the contrary, he says, "that if the midwife should pass her fingers in that direction, she would meet with *nothing but a little close bag*;" now, this "close bag" is the

vagina drawn forward from its natural position by the untoward situation of the womb, "which is raised *too high in the belly*, and is so obliquely situated, that its mouth will no longer be in a right line with the vagina." But if this "close bag" be pressed against, the head of the child can be felt through the parieties of the uterus; in Mrs. F.'s case however, the presentation could not be discovered by the most deliberate and careful research; there is nothing then in common between these two cases.

"Or how," continues Dr. M. "could the os uteri be so situated, as to make it almost impossible to touch it, unless the fundus was thrown down between the rectum and vagina?" To this it may be replied, that Deventer expressly declares, that the uterus was too high in the belly, and too strongly pressed against the back bone, in the case he is describing, and that this is the reason the mouth of the uterus is carried up to the ossa pubis—and to prove, that he considered this to be the situation of both fœtus and consequently uterus, we must again refer to the plates. If Deventer is describing from imagination, Dr. M. is certainly not authorised to employ to his advantage the fruits of it; his testimony must be received, as we have already said, either entirely or not at all.

"Or what," adds Dr. M. "except this position of the womb, could occasion the very great danger, which Deventer so feelingly laments?" If the position assigned to the uterus be granted, we can very easily imagine from whence the "danger" would arise—the head resting upon the margin of the pelvis at the ossa pubis in the manner described, it can readily be conceived, that both mother and child would be in jeopardy, since the uterine forces would tend but to confirm its situation there; the head would not descend into the cavity of the pelvis (and Deventer declares it will not in the case he is describing), and consequently the woman cannot be delivered without experienced aid.

"If the fundus of the gravid uterus," continues Dr. M. "at nine months merely rested against the back bone without being retroverted, how is it possible that the os uteri

should rise out of reach towards the cervix vesicæ?" To this we must first observe, that if the uterus were really retroverted, its fundus could not possibly rest against the "back bone," for it would necessarily be too low, and in an opposite direction—for it must in that situation, agreeably to Dr. M.'s own account, occupy the lower part of the pelvis; for he states that it can be felt through the rectum—now, the fundus cannot occupy both the hollow of the sacrum, and rest against the "back bone" at the same time; and secondly we must declare, that Deventer in no instance says the os uteri is "beyond reach," but on the contrary tells us its lower edge can be felt, by carrying the finger between it and the neck of the bladder.

Dr. M. says, "It might indeed" (the mouth of the uterus) "be turned more than usually forwards towards the pubes; but could not possibly *ascend out of reach* from this cause." Now, to this we need but repeat what we have this moment said, that Deventer does not say it does "ascend out of reach." "Besides," says Dr. M. "if this alone were the case, what should prevent the labour pains from restoring the os uteri to its proper situation in a few hours? This position of the womb might, indeed, occasion a tedious and hard labour, but could scarcely expose the life of the woman to much danger; whereas Deventer represents the situation of the womb as most *perilous*, in which both mother and child, after suffering a very long and painful labour, often lose their lives." To the latter we say, that when the inflection of the lumbar column is greater than ordinary, of consequence the projection of the sacrum increased, even without injuring the diameter of the upper strait from pubes to sacrum, the head of the child, if this should be the presenting part, will be made to rest upon the anterior margin of the pelvis, owing to the uterus rising higher, or rather perhaps, not being able to sink into the pelvis as it is wont to do in a healthy construction of these parts; for the axis of the fundus and the mouth of the uterus, in this case, will not correspond with that of the superior strait, and this is precisely what Deventer urges to be the situation in those labours he "so feelingly

laments." Should then, this want of correspondence of the axes of the uterus and pelvis obtain as we have just stated, it must be evident, that both mother and child will suffer greatly, unless timeously relieved. We must then, from what we have said, conclude, that Deventer's account of the posterior obliquity of the uterus affords no aid to Dr. M.'s hypothesis.*

We shall now proceed to consider Dr. M.'s two other modes of termination of retroverted uterus at full time.

"Secondly.—The want of some or all of these fortunate circumstances, or injudicious management during labour, may occasion the poor woman to fall a victim to this untoward position of the womb in the course of a few days, either by a rupture of the uterus producing speedy death, or by an active inflammation or mortification of the parts."

"Thirdly.—The uterus being unable to extricate itself out of the awkward position into which it was thrown, may passively submit to the burthen, until, by the slow process of ulceration, the fœtus may be excluded through the rectum or vagina, and the mother remain alive."

Dr. M. considers these two last terminations pretty much together, thinking the facts and arguments which may support one will aid the other. We shall follow him in the march he has laid down for himself. He begins with the history of a case from Patuna, and we must take it for granted he relates the case fairly, as we cannot lay our hands upon the original. He says, "The first which occurs to me as being of this kind is related by Nicholas Patuna,†

* Dr. King concurs with Dr. M. that the description given by Deventer "of some obliquities of the womb, were in whole or in part, cases of retroverted uteri." *Analysis*, p. 46. We must really suppose that Dr. K. has rested satisfied with Dr. M.'s extracts, and not have read for himself, by his consenting to such a conclusion.

† There is a case circumstantially related in the fifth volume, page 162, of the *Medico-Chirurgical Journal*, which is said by Professor Santorini to have happened in his own practice, but which we strongly suspect is the same as is related by Patuna. They agree in every particular, even to the accident which befel Patuna's patient on her journey to Venice, namely her receiving a severe shock from "the fall of her vehicle." It would seem a matter of downright impossibility that two cases should so strongly resemble each other in their detail, not

a surgeon of Venice, of a lady from whom he extracted, through the anus, the body of a child which had been retained twenty months in the abdomen of the mother. The circumstances which induce a belief that this was an instance of retroverted uterus are, *the suppression of urine in the early part of her pregnancy, and the great discharge from the uterus of serous fluid tinged with blood during the ineffectual pains of labour.*

“This lady had borne two children at the full time, and had one miscarriage. In October, 1724, she conceived again, and in six weeks after, she was attacked with violent pain in the lower belly, and was unable to pass her urine except when lying on her back. From this time until July, 1725, when she entered her ninth month of pregnancy, she suffered much at various times, from pains in the belly and loins, and other complaints. At the beginning of this month she had a profuse flow of milk, which ran out and kept her very uncomfortable. About the middle of the month, her reckoning being then completed, labour pains came on, and the birth of the child was every moment expected, but was not accomplished. Four or five days after this there was a great discharge from the uterus of a serous fluid tinged with blood, very putrid and offensive; and a draining continued many months after.”

only as regards the main objects of the relation, but in all and every peculiarity attending the case itself. Patuna's patient was pregnant for the fourth time, so was Santorini's. Patuna's patient suffered from abdominal pains and dysury, so did Santorini's. Patuna's patient suffered much until her ninth month, so did Santorini's—At this time in both, pains resembling labour came on, and the fœtus died most probably, and there was a plentiful secretion of milk, &c. In both there was a discharge of blood from the uterus, which became white and offensive. After a variety of sufferings, in the account of which they both agree, her health began to mend, when they also both agree, she became again pregnant. About the sixth month they both say the lady suffered from the breaking down of her carriage, and pain and much distress followed; that upon examination per anum there was discovered a hard sharp substance, which proved to be the parietal bones. They were extracted, and, after a time, the remains of a *female fœtus*. In about eight days after this they both say the placenta came away, which was both putrid and small; and both agree that in a short time after, the health of the lady was restored.—See Merriman, p. 44, and Recueil Period. de la Soc. de Med. de Paris, Aout 1817.

"Some months after, her general health began to improve, and she again became pregnant in July, 1726, and so continued until January, 1727, when she miscarried of a male child of the usual size at six months." "Being recovered from this miscarriage, she undertook a journey to Venice, during which she received a severe shock of the whole body from the carriage breaking down."

"After this she suffered considerably from fever, pain, diarrhœa, vomiting, &c. At this time she was first visited by Patuna. He examined her per vaginam; and found at the superior part of this canal a tumour which was hard and not very large. He examined also per anum, and there found a hard pointed substance which scratched his finger, which was cautiously extracted by a pair of small forceps. This proved to be one of the cranial bones of a fœtus. In a few days more he succeeded in delivering in one mass all the remaining portion of a female fœtus. Eight days after the placenta was excluded, very putrid and much diminished in size. The mother soon recovered."

Dr. M. confesses that "the suppression of urine in the early part of the pregnancy, and the coloured discharge at the full period, when an effort was made to relieve the patient," were the circumstances which induced him to believe this to be a case of retroversion;—there is an old saying, "we can easily believe what we wish to be true," and on this principle we reconcile Dr. M.'s facility of credence. We have, we trust, already said enough in our former paper on the uncertainty of these marks to characterize retroversion, to convince any unprejudiced mind, that they are not to be exclusively relied upon. And we could cite many authorities to prove, that suppression of urine is a common symptom in pregnant women, and that retroversion is comparatively a rare one; and that this inconvenience is frequently produced in the unimpregnated uterus by a prolapsus; and this and other causes may induce these symptoms in the early period of gestation, besides a retroversion; but as these facts are familiar to every obstetrical practitioner, we will consume no more time, in farther in-

sisting upon them. As regards "the great discharge from the uterus of serous fluid tinged with blood during the ineffectual pains of labour," we can readily show this to be no uncommon attendant upon indisputable cases of extra-uterine pregnancy, and by no means decidedly marking a *fœtus in utero*.*

The changes which the uterus itself undergoes in cases of extra-uterine conception, are similar in many instances to those produced when the *fœtus* is lodged within its cavity—that is, there is an increase of size, by a distention of its parietes, from an augmented influx of blood and a production of decidua, &c. Dr. Baillie† says that Dr. Hunter had a preparation of a tubal pregnancy in which the uterus was enlarged to twice its natural size and contained a decidua. Mr. Burns‡ says, "it is curious to observe, that generally the uterus enlarges somewhat in cases of extra-uterine conceptions, and in most instances I imagine decidua is formed." Dr. Clarke§ says, he found in a case of extra-uterine conception of two months, the uterus as large as if it contained the *fœtus*, and that decidua was formed. In the history of the case related by Mr. Blizard,|| it is stated that "changes had been produced in the uterus, similar to what are seen in ordinary gestation; its parietes were thickened; its cavity enlarged; its cervix shut up with a jelly; and the glutinous effusion just described might not perhaps improperly be considered as a *membrana decidua* in its early state of formation." It would be easy to multiply testimony on this head were it important to do so—but, as we consider the production of the decidua, indisputable, we shall rest satisfied with those we have now offered. It would seem then easy to account for a coloured fluid issuing from the uterus when the organization of this membrane should be injured, as it would necessarily be, by repeated uterine con-

* Dr. King also accounts for the dysury, &c. in this case by supposing the uterus was prolapsed. p. 49, *Analysis*, &c.

† *Phil. Trans.* vol. vii.

‡ *Principles of Midwifery*, p. 192, 5th edition.

§ *Trans. of a Society*, &c. vol. i. p. 216.

|| *Edin. Phil. Trans.* vol. v. p. 189.

tractions, and also for its continuance for a long time, even after pain had ceased, without supposing it essential to these ends, that the fœtus should be contained within its cavity. The vascularity of the decidua is put beyond doubt by injections; and the phenomena of abortion furnish further proof.

Besides, we have evidence of this discharge taking place in unequivocal instances of extra conception. In the case related by Mr. Turnbull,* at the end of seven months, there was a flooding which continued for four or five weeks, and a substance was discharged which was taken for placenta by the midwife, but which most probably was nothing more than the expulsion of the deciduous coat. In Mr. Hey's† case there was flooding at the ninth month; and continued for a month afterwards to be serous. In a case related by Dr. Lobstein,‡ at the end of the third month, coagula were discharged, then fluid blood, and then coloured serum. La Croix§ relates a case in which the discharge continued for a long time from the uterus. Lest it should be supposed, that certain of the symptoms which accompanied this case, were peculiar to uterine pregnancy, we shall advert to them, and prove by incontestable evidence, that they are necessary or at least usual attendants upon extra conception. And, First, of the "profuse flow of milk from her breasts, which ran out and kept her very uncomfortable." It would seem from the history of very many cases of indubitable extra-uterine conception, that this is almost a never failing attendant, and is recorded as such, wherever the instances are well detailed; and marks in these cases, as well as in those of uterine pregnancies, the death of the fœtus.|| It is almost an infallible guide in the latter kind, and we are disposed to believe it to be equally decisive on the other.

In Mr. Turnbull's¶ case, the breasts became hard and painful, and a butyraceous fluid was plentifully secreted. Mr

* Mem. Med. Soc. vol. iii. p. 176.

† Med. Obs. and Inq. vol. iii. p. 341.

‡ Med. and Chir. Jour. vol. iii. p. 415.

§ Fourcroy, *Medicine Eclairée*, tom. iv. p. 346.

|| Ibid.

¶ Mem. Med. Soc. vol. iii. p. 176.

Hey* reports his case to be accompanied with a secretion of milk. In Mr. Bell's† case milk was secreted, and continued to be secreted for several years. It would be easy to add to this evidence were it important. We may next notice several symptoms common to pregnancy, such as pains in the belly and loins, vomiting, &c. but all of which attend in extra-uterine conception. Indeed, the cases resemble each other so much, that it requires an examination per vaginam to determine the question. The quickening is also as evident in many instances of extra conception as in the uterine; but in the former, it is generally described as higher or confined to one side, &c.

Let us now advert to one very important part of this history, namely, "that she again became pregnant in July, 1726, and so continued until January, 1727, when she miscarried of a male fœtus, of the size that is usual at six months." This circumstance, if the case be regarded as one of the extra-uterine kind, is not very uncommon, for we have a number of similar instances upon record. Dr. King‡ relates a case in which this occurred, at the end of six years after the first conception, and during the sojourn of that fœtus in the abdomen. In Dr. Bard's§ case there was an uterine conception and delivery of a healthy child while a fœtus occupied the abdomen; besides many others. In several of these, the uterus is described after dissection, to be healthy and without blemish in its appearance. Now, were this a case of retroversion, as supposed by Dr. Merriman, and were we to adopt his explanation of such cases, we must believe the uterus capable of sustaining, without the interruption of its most important functions, injuries of the most extensive and destructive kind. It would be admitting it could bear the loss of large portions of its substance without any derangement of its economy. The process of ulceration, it would seem, could scarcely injure it, since it can take place to such an extent as to allow a fœtus of a size

* Med. Obs. and Enq. vol. iii. p. 341.

‡ Edin. Med. Essays, vol. v. p. 362.

† Med. Comment. vol. ii. p. 72.

§ Med. Obs. and Enq. vol. ii. p. 369.

that the sex could be discovered, together with its placenta, to pass through it, without destroying its future usefulness. Can we, or can Dr. M. himself, for an instant believe, that the uterus can sustain the loss of so considerable a portion of its fundus or body, and yet perform its functions as certainly and as healthily as if its integrity had never been impaired? We cannot then regard this case of Patuna's to make any thing in favour of Dr. M.'s explanation.

Dr. M. quotes a case from Bartholine, but on which he himself seems to lay but little stress; yet, in our opinion rather more than it deserves; for although Bartholine may have thought as he has written, still it is an opinion founded upon most slender and equivocal data. Bartholine is made to say, "there could be no doubt that it" (the fœtus) "was detained in the uterus, as there was a discharge of pus from the womb at the same time, and pains like labour had been felt about the pubes and groins."* In an investigation which involves so much, we cannot forbear expressing our surprise, that Dr. M. should have employed evidence so imperfect and doubtful; and the more especially if the facts were really what Dr. M. would wish them to be, still, they would prove nothing to the purpose. How was it ascertained that the discharge of "pus" was really from the uterus? It is scarcely to be believed that Bartholine took the trouble, by the employment of a speculum, to determine that the discharge really issued from the uterus; and if he did not, the source of the discharge may have been from the vaginal surface;† or the abscess formed by the irritation of the bones between the rectum and vagina, may have opened itself within that canal, since there would be no more difficulty in penetrating this way, than through the rectum. And as regards "pains like labour," it is well

* Bartholine does not by any means speak so positively on this subject as he is made to do by Dr. M. He merely states, "it would seem," &c. instead of saying "there could be no doubt," &c.

† The discharge from leucorrhœa so much resembles pus in many instances, that it would be difficult to discriminate between them; or the surface of the vagina may have secreted perhaps a genuine pus, or the uterus itself be subject to a purulent excretion, as suggested by Dr. King. Analysis, p. 58.

known that they attend cases of indisputable extra-uterine conceptions.

Dr. M. next brings a case from Perfect's cases in midwifery, vol. ii. p. 171. Where Dr. M'Kensie says, "I knew an instance of a child found *without the uterus in the abdomen of the mother*. The pains came on, and a midwife was employed; this woman, finding an enlargement in the vagina, mistook that for the membranes, which she attempted to break through by repeatedly scratching it with her nails, in which she succeeded so far as to evacuate the waters. However, the birth of the child being still retarded, a man midwife was procured, but to no purpose, the woman growing worse and worse, till at length she died."

This case is still less to the point than the one just mentioned from Bartholine—for Dr. M'Kensie declares "*the child was without the uterus in the abdomen of its mother*," consequently could not be a retroversion. But Dr. M. supposes Dr. M'K. was mistaken, we presume, from his having taken notice of this case, and most probably from the circumstance, that the midwife by scratching had "succeeded so far as to evacuate the waters." But is it reasonable to suppose that the midwife could simply by the use of her nail, have scraped through the vagina and uterus? we should regard her penetrating through the vagina and membranes which cover the child, a work of considerable difficulty, yet practicable, and in doing this, give issue to some fluid which may have immediately surrounded or touched the child; for in cases of acknowledged extra-uterine conceptions, the fœtus is surrounded by more or less fluid. In Dr. Giffard's* case a discharge of bloody water took place from the anus, before the child was delivered, which he expressly declares to be "the water in which the fœtus floated."† The mere discharge then of water is no way suffi-

* Midwifery, case 157, p. 575.

† Dr. King, analysis, p. 10 and 11, tells us in his case, that "the instant the vagina was laid open, the waters flowed abundantly, the membranes being laid open with the same incision."

cient to throw any doubt upon the nature of Dr. M.K.'s case.

The next case Dr. M. presents us with, is from Dr. Simpson.* On this case, his remarks we conceive are not justified by the detail of it. He begins by saying, "I think it not *improbable* that the case which occurred to Dr. Simpson was a case of this kind," (retroversion at full time.) Dr. S. informs us, "that the woman had once been delivered by the crotchet." This woman, Dr. S. states, had been in labour for several days, and was with considerable difficulty delivered, after opening the head, owing to the deformity of the pelvis. "In her second labour," continues Dr. M. "he could not find the smallest opening into the uterus, and, *therefore*, judged that the lips of the os uteri had grown together." "But what could have occasioned this coalescence," says Dr. M. in a note, "of the os uteri, is not explained; there is no account of any inflammation about these parts after the conception (the second he means) had taken place."

Dr. S. is unfairly quoted in this passage by Dr. M. He makes Dr. S. draw the conclusion "that because he could not find the smallest opening into the uterus," it must therefore follow "that the lips of the os uteri had grown together." We shall show, however, from Dr. S.'s own words that he drew no such inference; but that the declaration that the lips of the os uteri being grown together, it was from ocular demonstration. This woman became pregnant a second time, and at the ordinary period had the symptoms of labour; which, says Dr. S. was hard "for two days before I saw her." He was then informed by the midwife that "the inner orifice had yielded nothing; I left her half a day, and things remaining in the same way at my return, I examined her condition, and found that the os tinæ had not only not yielded, but that the sides of it were grown together without any vestige of a passage; whereupon I asked the assistance of another physician, and Dr. Haddow being called was satisfied, as well as the mid-

* Edin. Med. Ess. vol. iii. p. 241.

wife, that the case was such as I judged it to be." It was now agreed that an opening should be made into the os uteri by incision; but before this was done the vagina was dilated by means of a speculum; which enabled them "distinctly to see the cicatrice of the grown together parts." From this we find that it was not conjecture, but demonstration.

It is true, as Dr. M. states, that "there is no account of any inflammation about these parts after the (second) conception had taken place," but there is a very particular one, of both inflammation and suppuration following the first delivery, and we have no hesitation to believe that this produced the union of the lips of the os tincæ. Dr. S. informs us that this woman "had for several days after delivery passed a great many small rugged stones by the urethra, and at length after her urine had been stopped some time, her husband drew out of the urethra a large piece of thick membranous substance, three inches in length, and in some parts two inches broad; one side of it was covered with a crust of small sharp stones, the other side was inflamed and bloody." "The patient continued a long time with a plentiful suppuration about the pudenda." In this account we hear of sufficient inflammation, &c. to account for the agglutination of the mouth of the uterus. But it may be said by others, beside Dr.*M. that this could not have happened at the time we suppose, since the woman became again pregnant, which could not have happened had the mouth of the uterus been permanently closed. But this is all speculation, and founded upon an opinion, that has in our opinion been satisfactorily proved to be erroneous,* and this led Dr. M. into the persuasion that the inflammation must be posterior to conception to have produced this coalescence.

"As it was impossible," continues Dr. M. "to find any opening into the uterus, it was determined, in consultation,

* It is very generally believed that the male semen must be directly conveyed into the uterus that impregnation may take place; but this is abundantly disproved by Couper, Saumerez, Haighton, &c.

to make an incision with a scalpel through the part where it was supposed that the os uteri was situated." From this it would unquestionably be supposed, that Dr. S. had determined to make an incision at random; but when he speaks for himself, he appears certain of the nature of the part on which he is about to operate; for after stating, as mentioned above, that the cicatrice which united the edges of the os uteri could distinctly be seen, he adds, that "he could have easy access to it, and divide it; which," he says, "I did by an incision at least half an inch deep, before I pierced through the substance of the womb; then immediately introducing my finger at this wound, I touched the child's head, and felt the whole circumference of the passage hard like cartilage, which yielded nothing to several throes she had after the incision, so that I was obliged to guide a narrow-bladed scalpel with my finger, to make several incisions into this cartilaginous ring." Here we have every evidence that it was really the os uteri that was operated on, and not upon a part where "it was supposed that the os uteri was situated." For it is first declared, that the cicatrice was seen which united the edges of the os uteri; secondly, he speaks of the circumference of the passage; thirdly, he calls it a cartilaginous ring. This indurated condition of the os uteri is recognised by several writers upon midwifery. Baudelocque speaks of it in the following manner: "Sometimes the pad which constitutes the neck of the uterus in the latter periods of pregnancy and in the time of labour, is hard, scirrhus, incapable of any extension or dilatation, so as entirely to hinder the exit of the child."

Dr. King concurs with Dr. M. that this was a case of retroversion; but his reasoning on this subject we conceive is liable to great objection. He says, "The cartilaginous hardness mentioned, indicates that something more than the coats of the vagina were cut through. The convoluted vessels of the uterus might have given this sort of resistance to the knife; the peritoneum could not, unless slightly ossified." It is truly so that "more than the coats of the

vagina were cut through," or rather they were not cut through, but the uterus itself; and, agreeably to Dr. K.'s confession, the convoluted vessels of this viscus might give this sort of resistance, though we are entirely at a loss to comprehend how "convoluted vessels" should offer more resistance than a firm elastic membrane. But, as we are persuaded that the uterus itself was cut, we shall content ourselves with his concession, that its convoluted vessels might "give this sort of resistance," without any inquiry into his meaning: for to us, we are free to confess, it is totally unintelligible. He adds "I concur with Dr. M.'s induction, viz. that this was a case of retroversion, because the patient died in twenty-four hours, a result which we trust would not accompany the incision of the simple vagina."

Dr. M. as well as Dr. K. has attempted to take advantage of the speedy death of the patient; for he sneeringly observes, "It can hardly be necessary to add, that the mother died in twenty-four hours," (after the operation.) Dr. M. had previously condemned it, as "being performed in a most rude and slovenly manner," of which there is not the slightest evidence from Dr. Simpson's account of it. We have read this case with great care, and we cannot for a moment admit that the operation (that is, the division of the os uteri) had the smallest agency in her death; and we are the more especially confirmed in this, by Dr. Simpson's statement of the sequel. He tells us, "My patient, immediately after being put to bed, was seized with pleuritic pain, very high fever, and difficulty of breathing, which, coming on so soon after her being fatigued several days with hard labour, during which she slept none, *but drank much of every thing in the way*, appeared to me rather the cause of her death than any consequence of the incisions I had made." In this we heartily concur with Dr. S.; for it is a fact well known to accoucheurs, that the mouth of the uterus has been frequently divided without the slightest injury having resulted from it. Coutouly proposes it as a safe practice in cases of convulsions, &c. where the os uteri

refuses to dilate, and where it is highly important that delivery should speedily take place. He gives a number of cases to prove the efficacy and safety of his practice. See his *Memoire sur l'incision des Bords du Col de la Matrice*, &c.

"Now," continues Dr. M., "in this history one circumstance is mentioned, which goes strongly to prove that there was a retroversion of the womb. Dr. S. states that the liquor amnii made its escape early in the labour; and he imagines that this evacuation was effected through the urethra, between which and the uterus he supposes there was an opening, the effect of a former inflammation of the parts. Is it not much more probable that the liquor amnii was evacuated through the os uteri, tilted very high above the pubes, as in Mrs. F.'s case? and that the incision which Dr. S. made was not through the coalesced os uteri, but through the posterior part of the vagina and the fundus uteri?"

We will let Dr. S. explain for himself; and this will show how determined Dr. M. was to force this history to comport with his hypothesis. "The want of waters (at the time of cutting into the uterus) was some surprise, till I recollected that in the time of labour she told us they were passing, at which time I had the curiosity to make a strict observation, and found that what she called the waters passed by the urethra, which opened by three orifices; this with her having lost such a portion of the bladder formerly, and her being subject to the gravel, gave me ground to think there was some communication between these passages and the cavity of the uterus, above the os tincæ." Dr. S., it is seen, speaks positively as to the issue of water through the urethra. We are led to believe it was ocular evidence, and therefore he could scarcely be mistaken; and besides, his explanation is every way satisfactory of the probability of a communication between the uterus and bladder; since we could cite instances to prove, in cases of extra-uterine conception, that a communication has been made between the sac containing the fœtus and the blad-

der, and the bones of fœtuses have been passed through the urethra.

As to Dr. M.'s conjecture that "it may have passed through the os tincæ," which he *imagines* was tilted very high above the pubes, there is not the smallest foundation. In the whole history of this case there is no mention of the os tincæ not occupying its usual situation,* nor of any tumour filling up the vagina, as must necessarily have happened had it been a case of retroversion; nor is there the most remote resemblance between this history and that of Mrs. F.'s.

The next case in order is Mr. Kelson's,† which Dr. M. relates, with a suppression of two or three material points—we shall therefore give it from Mr. Kelson's own account.—He was sent to visit a Mrs. Townsend, who was suffering very considerable pain from a partial suppression of urine and fæces, and who supposed herself ten weeks with child, as she had missed two menstrual periods, "a very trifling appearance at the end of each month excepted." These symptoms were twice relieved by cathartics—in the course of a few days more, a total suppression of urine took place, and "upon examination," says Mr. K. "I discovered, as I then supposed, that it arose from a retroversion of the uterus, the lower part of the pelvis being completely filled with a hard tumour, and the os tincæ not to be found without much difficulty. I relieved her by the common female catheter, to which I was obliged to have recourse for a fortnight longer. At the end of that time, the impediment, as I had foretold, was suddenly removed, the uterus taking pretty much its natural situation." From this relation, we are of opinion, that the two first inconveniencies which Mrs. T. suffered were not occasioned by retroversion; and were nothing more than what is pretty frequent with women in the first periods of pregnancy, from prolapsus, and especially with those who are much upon their feet, and subjected

* On the contrary we are informed by Dr. S. that he really saw the os tincæ; now this would have been impossible were Dr. M.'s conjecture well founded.

† Med. and Phys. Journal, vol. xi. p. 293.

to more or less fatigue, as was most probably the case in this instance, as the patient was the wife of a farmer; and particularly as she was in both instances relieved by cathartic medicines. And, that total suppression did not take place until the vaginal tumour had acquired sufficient bulk to entirely compress the neck of the bladder; but which compression was eventually suddenly relieved by the tumour still augmenting, and rising higher in the pelvis, and thus freeing the bladder from the restraint which it had before imposed upon it, by pressing the uterus against it. And in this manner we may rationally and satisfactorily account for the *os tinæ* being in these cases always remote, sometimes inaccessible to the finger, and constantly behind the *ossa pubis*.

From this period she progressed in the ordinary manner of pregnancy—she became larger—the motion of the child was strong and frequent, but was felt “on each side, and at her back bone.” On this latter circumstance Dr. M. seems to lay some stress, for he says, “from this it may be inferred that the uterus did not *fully* acquire its natural position in the pelvis; and that the sensations of the mother, when the child moved, seemed to prove that it still remained retroverted.” Now, it is not asserted by Mr. K. that the uterus had acquired entirely its natural situation, but “pretty much its natural situation,” which is sufficient to prove, it was not in a state of retroversion, at the time indicated, and this is all that is necessary to contend for. As regards the stirrings of the child, it could easily be shown that a similar account is given in cases of decided extra-uterine conception, and we should claim it as a mark of such, did we feel we needed such support. Besides, the facts pointed out by Mrs. F. for the motion of her child, would go no way to prove a retroversion, since in this case the motion would be confined to the sacrum, and Mrs. F. complained it was at each side and at her back bone, the parts that would necessarily receive the sensation in case of extra-uterine pregnancy.

For a month after the beginning of January 1802, the

period at which the time of gestation would be completed, she was occasionally seized with violent pains resembling labour; these would intermit from time to time; Mr. K. did not examine his patient. But "after this," says Mr. K. "I visited her daily; she continued languid and unwell, and on the fourth day she had shiverings, succeeded with some feverish heat. Her breasts began filling with milk, and by no means in a small degree, for on the fifth day they were as powerfully distended as one usually meets with in a healthy woman who had been delivered that time. All these symptoms were more than sufficient to make me suspect, what upon examination evidently appeared to be the case, *that the fœtus was extra-uterine.*" From this history of symptoms, we are led to conclude, that the death of the fœtus took place with their commencement; and it would prepare us to anticipate the situation of the child as it did Mr. K.; and would prevent surprise, that an examination should prove the "fœtus to be extra-uterine." Dr. M. conceals this declaration of Mr. K., as it is a positive avowal of his opinion, and he most certainly had the best opportunity to judge.

"Upon the first and every other examination," says Mr. K., "I found the parts somewhat in confusion,* the child plainly to be felt through the vagina, the uterus not enlarged, but forced upwards and forward, the os tincæ quite closed; from this last of pain, all motion of the child ceased. A month after, she became regular in her female health, and has without interruption continued so to this time."

About fifteen months after this, her health began to suffer much—entirely deprived of appetite; night sweats; menstruation ceased; with a number of other alarming symptoms, together with a "sudden discharge of a large

* "Great must be the confusion," says Dr. King, p. 70, "to an accoucheur, when the os uteri is quite closed, and pressed upwards and forwards, and not enlarged with labour pains, and the child plainly to be felt through the vagina; let the reader compare this epitome with my paper, and I think he need not hesitate to decide, that this was also a case of ventral gestation."

quantity of very putrid slimy matter, from the anus," which continued at intervals for a month or more—and after a lapse of time, many bones were discharged from the same part.

Upon this case Dr. M. further remarks, " enough is told to satisfy the reader, that the uterus had been in a retroverted state, occasioning a suppression of urine ; that after the suppression was removed, the uterus had not completely regained its proper situation ; that it remained in an unto-ward position till the termination of the full period of utero-gestation, and that the pains being inadequate to the restoring it to its natural state, the parieties of the womb had, either from a laceration or from ulceration, given way, and allowed the escape of the fœtus into the hollow of the pelvis, between the vagina and rectum."

Upon these observations and Mr. K.'s history we shall offer the following additional remarks:—First, Should it be admitted that the uterus was in a state of retroversion in the earlier part of pregnancy, it is evident, from the whole account we have of this case, it was so in an unimpregnated state (as we have already remarked) and that it was suddenly restored to "nearly its natural position," agreeably to Mr. K.'s statement. Secondly, That after the cessation of the last pains an interval of but four days elapsed when she was attacked with symptoms which clearly expressed the death of the fœtus ; and that, upon examination, it could be distinctly felt through the vagina, a circumstance which could not have obtained, had it been enclosed within the uterus ; the uterus found enlarged and "forced upwards and forward," the os tincæ quite closed. Thirdly, That if the fœtus had changed place by a laceration of the parietes of the uterus, we should have had the train of symptoms which never fail to follow it ; but not one of these were present. The only symptoms recorded at this period were those consequent upon the death of the fœtus, and the secretion of the milk. Fourthly, That if the fœtus changed its situation by "ulceration," it must have happened in the course of four days, a period, Dr. M. himself would admit, far too short for this process :

besides, it was not accompanied by a single symptom which would declare it about to take place. Fifthly, The uterus, in this case, could not have suffered from either of these processes, as we are distinctly told, the patient menstruated regularly after a month, and continued to do so until the period of Mr. K.'s writing the first part of this account, an interval of fourteen months. Sixthly, That the symptoms which preceded the ulceration which permitted the fœtus to escape *ex ano*, were precisely such as always attend this process in these parts, and bear not the slightest resemblance to those which presented themselves at the period Dr. M. supposes the fœtus had escaped from the uterus by a similar process. Seventhly, The discharge which preceded the expulsion of the bones was exactly what would be expected on the rupture of a sac containing an extra-uterine fœtus, and which, perhaps, could not have happened, had it made its way through the parietes of the uterus, either by a rupture or ulceration of them. From all we have said we conclude, that this case does not in the smallest degree sustain Dr. M.'s hypothesis.*

The next case presented for our consideration is the one related by Mr. Coleman. It is as follows:—Mrs. Cooper, on the 25th December, 1798, sent for her midwife, who gave her assurances of a speedy delivery. She was at this time at the end of her reckoning. On the 7th January, 1799, Mr. Coleman was sent for. She had no pain;† but upon “examining her, a globular substance was found very low in the pelvis, which he supposed was the head of the child; but the *os uteri* could not be discovered.” He staid with her some time, and desired to be sent for should her labour come on. He, not hearing from her, called on her two months after, and found her still undelivered, and re-

* Dr. King, page 74, observes on this case, “We are obliged in a review of all circumstances of this case, to conclude that Dr. M.'s induction is not established.”

† “According to Dr. M.'s instances of Mrs. Wilkes and Mrs. F.'s cases, Mr. Coleman's patient should, in five, six, or in a few days, have manifested fresh signs of labour, and if it were really a case of retroversion, she should have been delivered, even of a dead child.”—*Analysis*, p. 90.

quantity of very putrid slimy matter, from the anus," which continued at intervals for a month or more—and after a lapse of time, many bones were discharged from the same part.

Upon this case Dr. M. further remarks, " enough is told to satisfy the reader, that the uterus had been in a retroverted state, occasioning a suppression of urine ; that after the suppression was removed, the uterus had not completely regained its proper situation ; that it remained in an untoward position till the termination of the full period of uterogestation, and that the pains being inadequate to the restoring it to its natural state, the parieties of the womb had, either from a laceration or from ulceration, given way, and allowed the escape of the fœtus into the hollow of the pelvis, between the vagina and rectum."

Upon these observations and Mr. K.'s history we shall offer the following additional remarks:—First, Should it be admitted that the uterus was in a state of retroversion in the earlier part of pregnancy, it is evident, from the whole account we have of this case, it was so in an unimpregnated state (as we have already remarked) and that it was suddenly restored to "nearly its natural position," agreeably to Mr. K.'s statement. Secondly, That after the cessation of the last pains an interval of but four days elapsed when she was attacked with symptoms which clearly expressed the death of the fœtus ; and that, upon examination, it could be distinctly felt through the vagina, a circumstance which could not have obtained, had it been enclosed within the uterus ; the uterus found enlarged and "forced upwards and forward," the os tincæ quite closed. Thirdly, That if the fœtus had changed place by a laceration of the parietes of the uterus, we should have had the train of symptoms which never fail to follow it ; but not one of these were present. The only symptoms recorded at this period were those consequent upon the death of the fœtus, and the secretion of the milk. Fourthly, That if the fœtus changed its situation by "ulceration," it must have happened in the course of four days, a period, Dr. M. himself would admit, far too short for this process ;

besides, it was not accompanied by a single symptom which would declare it about to take place. Fifthly, The uterus, in this case, could not have suffered from either of these processes, as we are distinctly told, the patient menstruated regularly after a month, and continued to do so until the period of Mr. K.'s writing the first part of this account, an interval of fourteen months. Sixthly, That the symptoms which preceded the ulceration which permitted the fœtus to escape *ex ano*, were precisely such as always attend this process in these parts, and bear not the slightest resemblance to those which presented themselves at the period Dr. M. supposes the fœtus had escaped from the uterus by a similar process. Seventhly, The discharge which preceded the expulsion of the bones was exactly what would be expected on the rupture of a sac containing an extra-uterine fœtus, and which, perhaps, could not have happened, had it made its way through the parietes of the uterus, either by a rupture or ulceration of them. From all we have said we conclude, that this case does not in the smallest degree sustain Dr. M.'s hypothesis.*

The next case presented for our consideration is the one related by Mr. Coleman. It is as follows:—Mrs. Cooper, on the 25th December, 1798, sent for her midwife, who gave her assurances of a speedy delivery. She was at this time at the end of her reckoning. On the 7th January, 1799, Mr. Coleman was sent for. She had no pain;† but upon “examining her, a globular substance was found very low in the pelvis, which he supposed was the head of the child; but the *os uteri* could not be discovered.” He staid with her some time, and desired to be sent for should her labour come on. He, not hearing from her, called on her two months after, and found her still undelivered, and re-

* Dr. King, page 74, observes on this case, “We are obliged in a review of all circumstances of this case, to conclude that Dr. M.'s induction is not established.”

† “According to Dr. M.'s instances of Mrs. Wilkes and Mrs. F.'s cases, Mr. Coleman's patient should, in five, six, or in a few days, have manifested fresh signs of labour, and if it were really a case of retroversion, she should have been delivered, even of a dead child.”—*Analysis*, p. 90.

ceived from her the following account. "She told him she had felt nothing of the child since Christmas day, but was certain she had previous to that time, though different from her sensations on former occasions." "The body," says Mr. C., "had nearly the same appearance as in natural pregnancy, with an unevenness above the os pubis. The whole had not exactly the usual globular form of the impregnated uterus." "I found the child's head," continues Mr. C., "pressing down very low, and could not discover the os tinæ in its usual situation, but thought I discovered it above the os pubis. On endeavouring to pass the finger towards the sacrum, it could not pass, owing to the vagina obstructing it in every direction backwards. I could pass the finger very high by the pubis, in which situation I found the os tinæ as before described." "I examined her again, and concluded it was an extra-uterine fœtus, lying between the rectum and the womb, pressing the uterus up against, and chiefly above the pubis."

Mr. Rigby now saw her, and had seen her in the earlier part of her pregnancy, but was not fully confirmed it was extra-uterine, though there was something extraordinary in the case. Her health, at the period of four months after her full period, had suffered much, and she was reduced to a most deplorable situation. On examining her now, Mr. C. says "I found an opening unlike the os uteri, and my finger passed immediately into the head of the child. She had ejected during the night a considerable quantity of fœtid bloody water. She had no pain but what I gave her, as I used some force, pressing upon the inside of the bones of the cranium, and endeavouring to dilate the opening." She was at last, by different efforts, delivered of a "fœtus in a highly putrid state. It appeared to be a male child at full time when it died, both from the formation of the bones, and the size of the fœtus." "The opening in the vagina, through which the fœtus passed, extended nearly to the neck of the uterus. We could now distinguish the neck of the uterus, and the uterus itself by the touch, the finger passing backwards into the large cavity from which

the child was extracted.* There was no doubt of a communication between the bowel and the cavity, as some seeds from a cake eaten the day before came away on Mr. Aldhouse's fingers, with a portion of *fæces*." After much suffering she was restored to tolerable health.

Dr. M. says, "this case is a valuable addition to the series of facts, by which I hope to prove, that retroversion of the uterus, at the full period of pregnancy is no very uncommon occurrence."

On this case we shall remark first, that we have not a word of information of the situation of Mrs. Cooper previously to Mr. Coleman's visit on the 7th January, 1799; and of course not made acquainted with any fact that would lead to the suspicion or declare that she had suffered a retroversion in the earlier part of her pregnancy. Secondly, that this silence would seem to declare, that this condition of the uterus did not exist, and the more especially, as she had been "visited by Mr. Rigby (whose opinions upon these subjects are much to be respected) in the earlier part of her pregnancy," who though not fully confirmed 'that it was extra-uterine,' yet as far as we can collect from Mr. Coleman's statement, did not intimate a suspicion of its being a case of retroversion at full time, but granted it was one in which there "was something extraordinary." Thirdly, that Mr. C.'s description of this case precisely corresponds with many we have upon record, where dissection proved them to be original pregnancies of the extra-uterine kind; in which the *os tincæ* could not be felt at all, or very imperfectly, and that only behind the *ossa pubis*—we have already noticed this condition of the uterus in our paper, page 247, to which we must now refer. Fourthly, that it is impossible from the nature of things that this could be a case of retroversion, since Dr. M. admits but two modes in which the *fœtus* can escape from the uterus, namely: by a rupture or by ulceration;—as respects the first of these modes, there is not a solitary symptom re-

* Dr. King thinks the whole of the *fœtal* mass would have passed through the anus, "had not the midwife injured the vagina."

corded, that could give rise to the suspicion that it had taken place; for from this accident Dr. M. himself allows, "the poor woman would fall a victim, in the course of a few days;" and as regards the second, there is as little room to admit it, as the first; for we are distinctly told, that it was a fœtus at full time, as was proved by the conformation of the bones and of the size of the child itself; and also informed that after its extraction, both "the neck of the uterus and the uterus itself, could be distinguished by the touch." Now, can it be supposed, that the uterus could "be distinguished," after it had suffered a lesion from ulceration, so extensive as to allow a fœtus and its appurtenances at full time to pass through it? Fifthly, the "ejection of a considerable quantity of fœtid bloody water," is precisely what occurs, as we have already noticed, where the fœtus is contained in an extra-uterine sac. From these considerations, we cannot withhold an expression of surprise, that Dr. M. should consider this case as auxiliary to his explanations; for to us it appears decidedly hostile, if not fatal to them.

The next case Dr. M. adduces, is that of Mrs. Mainwaring;* the history of this case differs so little in essentials from the many we have already detailed, that we shall confine ourselves to such parts only as Dr. M. lays a stress upon—for we cannot but regard it as decidedly extra-uterine. The first is, "the patient complaining of a sense of fulness, and a frequent and strong desire to go to stool, and passing with much difficulty but small quantities of urine." We have already remarked sufficiently at length upon these symptoms, and we need not repeat here; the fact is too notorious that these symptoms do not constitute a retroversion, and that they are frequently induced by a prolapsus uteri. The second is, "that when she had attained between the second and third month of her pregnancy, she had strong symptoms of being about to miscarry, but no ovum came away"—in several of the histories of extra-uterine pregnancies, this circumstance is particularly noticed; so

* Trans. of a Soc. for Imp. of Med. Knowl. vol. ii. p. 287.

that it does not appear, that pain or even a considerable discharge of blood from the uterus, prove the presence of an ovum within its cavity. The third is, "that a tumour in each groin, and which extended almost as high as the navel, were discovered—they were painful upon the slightest pressure." But are tumours in the groins symptoms of a retroverted uterus; we know of no authority for this. These tumours we should rather believe would make against the idea of retroversion, since we are told that one of them "reached almost as high as the navel," and this must have been before the fifth month of pregnancy—now, had this been a retroversion, no such thing could exist, for at the period indicated, the uterus would necessarily be entirely confined to the pelvic cavity; whereas in this case the tumour was higher than in an uterine pregnancy. The fourth is, that "a coffee coloured fluid was now discharged from the urinary bladder, in quantity nearly amounting to three pints in twenty-four hours." But "before this period the quantity was less." Dr. M. wishes to show this to be a symptom of retroversion, by supposing that "such symptoms would very probably be present in a case of retroversion, where the flow of urine was considerably impeded by the uterine tumour, but not entirely suppressed." But unfortunately for this conjecture, it is stated, that "a quantity amounting to three pints was discharged, (though before that precise period there was less,) in the course of twenty-four hours." Now, will any body declare, the flow of urine to be considerably impeded, where at least a quart per diem is discharged? At all events it would prevent the bladder from sustaining any injury from distention, and that is all that is necessary to contend for in this instance, as it is on this principle Dr. M. would account for this high coloured urine. As regards this we may remark, that the rupture of a small vessel within the bladder, a circumstance not unlikely to happen, would satisfactorily explain this appearance.*

* Dr. King declares he has known this to happen in several instances. p. 100 of Analysis, &c.

"Let us now advert," says Dr. M., "to what was discovered by an examination per vaginam, at about the sixth month of her pregnancy."

"A tumour was found in the hollow of the sacrum, occupying its whole extent, and projecting so much forward as nearly to fill the cavity of the pelvis. It seemed to lie between the vagina and rectum, and was less than two inches within the pelvis, reckoning from the external orifice."

"The os uteri was altered in its shape and situation, being pressed against the bladder and pubes. The cervix uteri was so fixed in its situation, as to resist any attempt which was made to move it upwards. From these circumstances, the urine was passed with difficulty."

"The shape of the tumour in the hollow of the sacrum was nearly round, but somewhat flattened upon the anterior part. In breadth, it was supposed to be between three and four inches, and in thickness from two to three. It felt moderately firm."

On the portions of Mr. Mainwaring's history, which Dr. M. felt it important to quote particularly, we shall only remark, that we have already said enough on the presence of a tumour in the pelvis, as above described, with all its consequences, to prove its total insufficiency to constitute a retroversion. And, that Mr. M. himself has declared, "that the tumour in the pelvis was lower than that which is found in cases of retroverted uterus; and that the posterior part of the vagina was without the puckering occasioned in that disease by the fundus falling down behind the vagina." On these observations, however, of Mr. M., Dr. M. remarks, that they "cannot be looked upon as proofs that the tumour was not occasioned by the retroversion of the womb." "For," continues he, "the situation of the tumour in the vagina, described before in Mrs. F.'s case, was much lower than ordinary in retroversion." But what does this prove? certainly nothing, since Dr. M. has not satisfactorily shown Mrs. F.'s to be a case in point.

"After a long series of sufferings, this poor woman's case terminated favourably, by the discharge of the bones, &c. of

a fœtus, through the rectum." And it was found upon examination, that "the projecting part of the cervix uteri into the vagina (about eighteen months after), was shorter upon the left side than upon the right. The vagina might be said to be somewhat constricted upon the left side, so that the cervix uteri was less moveable there than upon the right, and some pain was felt in touching this part with the finger." "Now this," says Dr. M., "I think is an additional proof, that the fœtus was at first contained within the womb, which being pressed down in a retroverted state between the vagina and rectum, and unable to extricate itself from that position, underwent the process of inflammation, adhesion and suppuration, and thus was relieved of its contents; but was consequently diminished in size."

On these concluding remarks of Dr. M. we shall briefly observe, that all the objections we have already urged against either rupture or ulceration of the womb in these cases, are in full force against them; and we shall rely upon our former arguments to prove Mr. Coleman's to be a case of extra-uterine conception; and that the contraction on the left side of the os tinæ can satisfactorily be accounted for, in the healing of the wound in the vagina, without the smallest necessity of supposing the health of this part to be involved in the destruction occasioned by ulceration.*

Having given our reasons for dissenting from Dr. M.'s facts and inferences, we shall conclude by stating what has been proposed and effected for the relief of the woman who may be the subject of an extra-uterine conception. They may be divided into two general heads, viz. First, To attempt the liberation of the fœtus by an operation; or, Secondly, to exclusively trust to the powers of nature. As far as we have been able to ascertain, no very correct or decided opinion upon the subject of an operation to relieve an extra-uterine fœtus was entertained previously to the time of Dr. Charles Kelly,† who, having witnessed the death

* "I must, therefore," says Dr. King, "dissent from Dr. M.'s inference, and coincide with Mr. Coleman, that this was an indubitable case of ventral gestation."—p. 97.

† *Med. Obs. and Enq.* vol. iii.

of a patient under these circumstances, in the year 1756, declares that in his opinion "the only method that would promise success in such a case, is to make an incision through the posterior part of the vagina, against which the head is strongly pressed, and which, in consequence of that pressure, becomes very thin. The child may, by such means, be easily extracted." Since that time this operation has been several times performed with varied success:—by Lauverjat,* with success to the mother; by De Lisle,† with success to the child;‡ by Novara.§ with success to the child;|| by Dr. King,¶ with entire success to both.

The method originally proposed by Dr. Kelly, and adopted by Dr. King, appears to have decided advantages over the method pursued by Novara, and recommended by others, whenever the child is forced into the pelvis, and can be felt through the vagina or rectum. It is then a simple operation, requiring only an incision through the coats of the vagina, at the part most tense or protruded. By this operation the risk of peritoneal inflammation is much diminished, and the wound itself no way so serious. We cannot but recommend the reading of his Analysis to every medical practitioner, or at all events the case itself as published in the Medical Repository of New-York. But it should be observed, and this is judiciously insisted upon by Dr. King, that the operation should be as speedily performed as the nature of the case will permit; for by an early operation, both mother and child may be preserved.

As regards the second mode, we have but little to say; for as we believe Dr. M. to have erred in his premises, we cannot entertain much respect for his deductions; and were

* Sabatier Med. Operat. tom. i. p. 316.

† Journal des Science, tom. x. p. 384.

‡ That is, the child was born alive, though it survived its birth but three quarters of an hour. It was premature, and supposed to be between the sixth and seventh months.

§ Ibid. tom. iii. p. 119.

|| This was, however, by gastrotomy. The mother died on the thirty-third day after the operation, and her death rather attributable to the delay than to it.

¶ Analysis, &c.

we even to grant the legitimacy of both, we should unhesitatingly reject them for ourselves; for to us it really appears but a plan to see how much poor human nature can bear before aid would be considered justifiable.

Dr. M.* considers the subject of extra-uterine conception further; but as his speculations upon this subject, are not necessarily connected with our present object, we shall not notice them now, but shall reserve the right to animadvert upon them separate and apart, at a future time.

ART. VII. *The History and Treatment of Bony Tumours.* By
WILLIAM GIBSON, M. D.

IN health a balance exists, between the absorbent and exhalent vessels, which preserves each part of the animal frame within regular bounds. Deviations from this natural standard, in many instances, produce diseases, differing in situation, degree and character, from the slightest perceptible change of organization, to the most unlimited extent of morbid structure. Hence two opposite classes of disease result—according as the laws which regulate the decomposition or increment of the body predominate. In one case, the constituent particles are removed, and diminution is the consequence; in the other, an undue proportion of matter accumulates; giving rise to general enlargement or to excessive deposition, in some individual texture. In this way every tumour must form. A tumour, whether formed of cellular membrane, skin, muscle, or bone, is a vascular organized mass; receiving a supply of homogeneous particles from the texture upon which it is reared, or with which it is assimilated. If this view be correct, we may perhaps conclude that such diseases, in their incipient state or original formation, do not differ in composition from the organ or part from which they are derived; but acquire specific characters, as they increase, from peculiarities of constitution or from local causes, the

* p. 66

operations of which are not always easily explained. That diseased superstructures, or "new productions which made no part of the original composition of the body,"* never exist, we cannot pretend to assert; but, we are inclined to consider such formations extremely rare.—The immediate foundation of every tumour, is probably laid in a bed of coagulable lymph; this lymph is rendered vascular, increases in every direction, in proportion to its supply, becomes smooth or irregular, soft or hard, fleshy or bony, parenchymatous, or medullary, according to the original structure, from which it is derived. How it is enabled to assume specific, vitiated, or malignant characters, cannot satisfactorily be explained, so long as our knowledge of the operations of nutrition and secretion are confined within narrow limits. But it is not our purpose, now, to speculate; at a future period, we propose to enter upon the pathology of bony tumours; at present, only to furnish their history and treatment—commencing with the subject of exostosis.

Exostosis.

Exostosis appears, generally, as a distinct tumour of a bone, is smooth on the surface, seldom painful, and may remain long without becoming very large. Occasionally, the whole surface of a bone is found ragged or scabrous; consisting of numerous plates laid over each other, and sometimes piled to a considerable height in thick irregular masses. Again—the disease appears in the form of tuberculated knobs or irregular excrescences, closely connected, or insulated. In a third variety, spinous processes or bony spires, are eked out several inches and terminate either in a sharp point or a knob. After careful examination of diseased bones in the splendid cabinet of the late Dr. Wistar, and of numerous specimens in my own collection, I have met with no other forms of exostosis, than those mentioned. I conclude, therefore, that the disease seldom appears in any other shape, and under this impression shall denominate

* See Abernethy's Surgical Works, vol. ii.

each variety, according to its external characters. The first I shall call *circumscribed* exostosis, the second, *lamelated* exostosis, the third *tuberculated* exostosis, and the fourth *spinous* exostosis.

Under the term exostosis, authors comprehend several diseases, essentially different in symptoms, structure and appearances. Thus we have the "*fungous exostosis*," which according to the account given of it by Mr. Astley Cooper,* must be the osteo-sarcoma or osteo-steatoma, complaints specific in their nature and in structure very remote from exostosis. The same may be said of *spina ventosa*. The divisions made by the French writers, founded upon supposed causes of the disease, such as the scrophulous, darterous, syphilitic, scorbutic, and carcinomatous exostosis, are equally faulty, since they do not designate any variation of structure or form, but only imaginary shades of difference. Upon the whole, then, we are inclined to consider exostosis as a mere enlargement, from inordinate deposition of ossific matter, transcending the natural limits of a bone, in the shape of a spherical tumour, a plaited covering, an irregular excrescence, or a pointed production; in each of which, however different the figure, the structure is the same.

The bones are all subject to exostosis;—sometimes the disease prevails universally. An instance is related by Mr. Abernethy "of a boy who was so excessively afflicted with an apparent predisposition to exostosis or an exuberant deposition of bony matter, that a very trifling blow would occasion a bony swelling on any bone of his body. His ligamentum nuchæ was ossified and prevented the motion of his neck; the margins of his axillæ were also ossified, so that he was, as it were, completely pinioned. Besides all this, the subject in question had numerous other exostoses on various parts of his body."† Saucerotte‡ has detailed the history of a man thirty-five years of age, in whom all

* Cooper and Travers' Surgical Essays, part 1st, p. 170.

† Cooper's Dictionary of Surgery, vol. i. p. 288.

‡ Medical and Physical Journal, vol. i. p. 491.

the bones except the teeth, were increased to double their natural thickness, but remained of the ordinary length. The skull became so large as to present a hideous appearance and the eyes were forced from the sockets. The ribs were approximated, and the sternum, clavicles and scapulæ, uncommonly prominent. During the increase of the bones the patient could not move himself, breathed with great difficulty, and suffered from pain in every part. The pulse was scarcely perceptible, and the urine deposited a thick white sediment. After suffering for seven years he died, but permission could not be obtained to examine the body.—Such is sometimes, the predisposition to exostosis, that even the muscles and tendons are completely ossified. A striking example of this kind is recorded by Dr. Henry.* The patient was a labourer nineteen years of age, and first perceived a painful swelling in his right wrist which gradually increased, involving all the muscles of the forearm and converting them into solid bone, as high as the elbow. The left arm was attacked in a similar manner; then the right leg from the ankle to the knee, and finally the disease extended from the shoulder to the fingers of each arm.

Of the individual bones, the cranium, lower jaw, ribs, sternum, and long bones of the extremities, are particularly liable to suffer from exostosis.

The frontal, parietal and occipital bones, are sometimes irregularly thickened by ossific depositions; at other times they are changed into regular plates of uncommon thickness. There is a skull in the possession of Dr. Davis, of this city, taken from a young negro, in which all these bones are upwards of an inch in thickness, and extremely compact and solid. Examples nearly similar are given by Sandifort.† The os frontis is more subject to exostosis than either the parietal or occipital bones. Frequently the external table is covered with bony excrescences, composed of spicula, resembling crystallizations.‡ Occasionally both tables are

* Philosophical Transactions, vol. xxi. p. 89.

† Museum Anatomicum, vol ii p 13

‡ Baillie's Engravings of Morbid Anatomy, p. 208.

involved, and encroach upon the dura mater and brain. Every variety of exostosis has been found on the skull; but the lamellated is the most common.

Exostosis may occupy the orbit, and become so large as to displace the eye. A few cases of the kind are recorded by writers. In the *Edinburgh Medical and Surgical Journal*,* an interesting history is furnished by Mr. Lucas, of a bony tumour an inch and a half in length, two inches five eighths in circumference, and weighing an ounce and two drams; which grew from the orbit of a woman twenty-eight years old, and forced the eye forwards and outwards—causing it to hang beyond the exterior edge of the socket. The disease was produced by a blow from a cow's horn, on the upper and inner angle of the orbit, nearly on the transverse suture. Mr. Lucas divided the upper eyelid and exposed the bone, which was so firmly fixed as to withstand every effort to extract it. The wound did not heal, and the bone continued to increase for some time. At length it became carious, and was finally drawn away, but not without difficulty. The eye was placed in its natural situation; and the sight, although nearly destroyed by pressure, was soon completely restored. Upon analysis, the tumour was found to consist of cartilage, phosphate of lime and carbonate of lime, was extremely irregular, resembled a wedge cut out of a sphere, and so hard as to admit of being polished like ivory.—A very striking example of inordinate deposition of bony matter, upon the orbits and adjacent parts, destructive of each eye, is recorded by Howship.† A man aged 50 took cold; inflammation supervened, and a tumour of the size of a hazle nut formed below the inner angle of each eye. These continued to enlarge, and were attended with intolerable pain, which extended to the bones of the face and head. The eyes were constantly inflamed and pressed forward in proportion as they were encroached upon, until they were displaced and burst. When examined by Mr. Howship in 1815, the patient, independent of loss of sight, and occasional head-ache, enjoyed good health. The tumours of

* Vol. i. p. 405. † *Practical Observations in Surgery*, p. 26.

the maxillary bones were as hard as ivory and not in the least painful when pressed, filled the orbits and inclosed the nasal bones so as to obliterate the nostrils.—A large exostosis of the consistence of ivory and filling the left orbit is represented in Dr. Baillie's plates of morbid anatomy.

The facial bones are frequently the seat of exostosis. "We have," says Mr. A. Cooper, "in the collection at St. Thomas' Hospital, a skull which I took from a fish woman who died in that hospital, who had long been remarkable (even at Billingsgate) for her hideous appearance. Two large swellings had been formed under the orbits in the forepart of her cheeks, between which the nose appeared wedged and the nostrils were closed; each eye projected considerably from its socket. This person was seized with a fit, which seemed to be of an apoplectic nature, and in that state was brought to St. Thomas' hospital, where she almost immediately died. Upon examination of the head an exostosis was found growing from each antrum, and forming the large swellings upon the cheeks: these also projected into the orbits so as to occasion the protrusion of the eyes. On the left side the exostosis entered the cranium, projected inwards through the orbital process of the os frontis, and occasioning such pressure upon the brain, as under a considerable excitement of the vessels of that organ, to produce apoplexy, which proved fatal to her."* An exostosis sometimes arises from the cavity of the antrum maxillare. At other times the polypus or fungus which sprouts from the lining membrane of that sinus is converted into a hard and incompressible exostosis. Mr. Abernethy† has furnished a very curious case of this description. The patient suffered more than nine years with a fungus, which bled profusely, but finally sloughed during a fever. A bony mass, soon rose upon the edges of the walls of the antrum, projected in a sloping form, and resembled a large tea-cup, fastened upon the face. The tumour was irregular on the

* Surgical Essays, part i. p. 171.

† Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. ii. p. 307.

surface and became too large to admit of extirpation. I am informed by Dr. Physick, that a case of fungus of the antrum, which terminated in a bony tumour, occurred to the late Dr. Jones of this city. Bordenave* has cited two cases of exostosis of the antrum, of enormous magnitude and of very solid texture. In one the patient was cured by the knife and cautery. The other was too large to admit of an operation, and after death, was found, upon dissection, smooth, polished, and extremely hard, on the surface, less compact internally and resembling very much the structure of pumice stone. Boyer† relates the case of a soldier who suffered upwards of ten years with an exostosis of the left maxillary sinus, which produced epiphora, pushed the eye forward, displaced the nose and palate bones and rendered the visage truly hideous. The disease proceeded from syphilis, and was at last cured by the long continued use of corrosive sublimate and the liquor of Van Swieten. Many examples of supposed exostosis of the antrum have been furnished by authors; there is reason to believe, however, that most of the cases described as such, have really been fungous tumours mixed with bony particles, or surrounded by bony coverings, as in osteo-sarcoma and spina ventosa—diseases essentially different from exostosis. The monstrous tumour of the antrum contained in the Heaviside museum, and described by Howship and Fox,‡ and the interesting specimen delineated by Sandifort,§ in his account of the collections of Albinus, Rau, and Van Doveren, were undoubtedly fleshy excrescences incorporated with considerable portions of bony matter.

The alveolar processes of the upper jaw are subject to exostosis. In several instances I have removed from the gums, tumours, partly sarcomatous and partly bony. In one case, I took from the alveolar processes of the left side of the upper jaw of a negro woman, forty years old, a very

* *Memoires de L'Academie Royale de Chirurgie*, tom. xiii. p. 408.

† *Traité des Maladies Chirurgicales*, tom vi. p. 168.

‡ See Howship's *Observations in Surgery*, and Fox on the Teeth.

§ *Museum Anatomicum*, vol. ii.

solid exostosis, which carried before it several of the teeth. The tumour equalled in size a walnut, and was so completely identified with the jaw, as only to be removed by Hey's saw and a strong bistoury. The complaint never returned. Cullerier* gives the history of an exostosis seated on the right side of the upper jaw just above the canine teeth and the adjoining molares. Pelletan supposed the tumour to contain a fluid, while Dubois considered it a fungus from the gums. It turned out, however, a very solid exostosis, and was successfully removed by the chisel and mallet.

In the lower jaw I have seen both the circumscribed and tuberculated exostosis. The former is most common and generally seated between the angle and symphysis. In the splendid work on the bones, by Chesselden,† there is a fine representation of an enormous irregular exostosis, which originated from fracture, gradually increased for twenty years, and then caused the patient's death. Gooch‡ relates the case of a woman, whose lower jaw was enlarged in its whole substance from the angle to the chin, was very prominent, and measured fifteen inches in circumference from those points: notwithstanding its magnitude the tumour occasioned very little impediment to the motion of the jaw; nor was it painful. It had been growing between two and three years, and arose without external injury. The patient enjoyed perfect health. Several French writers, especially Bordenave,§ have described, under the title of exostosis, very large bony tumours of the lower jaw; but in every instance, so far as can be determined from their histories, they have appeared to me cases of spina ventosa or of osteo-sarcoma, and as such shall be noticed hereafter.

The humerus is subject to every variety of exostosis. The circumscribed and spinous are most common. Several specimens, in the Wistar Cabinet, have these varieties blended. In four examples the bone is uniformly enlarged, from

* Dictionnaire des Sciences Medicales, tom. 14 p. 228.

† Osteographia, or the Anatomy of the Bones, Tab. XLII. fig. 2.

‡ Chirurgical works, vol II. p. 69.

§ Memoires de l'Academie, tom. XIV. p. 125.

near its head almost to the middle, and from this enlargement several spinous processes above an inch long project. (Plate II. fig. 1.) The condyles and head of the bone are free from disease.—Sandifort* has described and delineated an exostosis which arises at the middle of the humerus by a narrow neck, forms an arch, and is then implanted by a bulbous extremity into the shaft of the bone—leaving a space between it and the tumour. In other examples, by the same writer, several spinous processes start in a cluster from the os humeri, immediately below its head. Again, single tuberculated exostoses spring from the neck of the humerus and resemble the great trochanter of the thigh. Several circumscribed exostoses, also of considerable magnitude, are described as occupying half of the humerus.

Exostosis of the scapula is extremely rare. Mr. Cooper† never met with the disease. Sandifort‡ mentions an instance where bony depositions, in great profusion, covered the acromion and coracoid processes and surrounded the head of the humerus.

The bones of the forearm are seldom affected by exostosis. I have three specimens in my collection—two of the ulna and one of the radius. The tumours are smooth, circumscribed, seated about the middle of the bones, and the size of a walnut. I have never met with genuine exostosis of the metacarpal bones or fingers, although spina ventosa of these bones frequently occurs, as will be shown subsequently.

Venereal exostosis of the clavicle is by no means unfrequent; but I have seen one instance only where it was independent of that complaint. The tumour was larger than an egg, circumscribed and smooth, occupied the middle of the clavicle, and arose from external injury. The patient was a labourer, forty years of age, and sustained no inconvenience from the swelling.

The ribs are sometimes studded over with small exostoses, and in some rare instances considerable bony masses are

* Museum Anatomicum.

† Surgical Essays, p. 174.

‡ Museum Anatomicum, vol. I. p. 222.

thrown out between the intercostal spaces. There is a specimen of this kind in my collection, which was found by Dr. Lawrance, on the battle ground near New Orleans. Dr. Godman, also, lecturer on anatomy in this city, has a very beautiful preparation, taken from one of the inferior animals, illustrative of the effect of external injury in the production of ossific deposition—bony plates being thrown out on the external and internal surface of the chest, forming a complete case and uniting several of the ribs closely together.—A very interesting case is related by Mr. Giffard,* of a patient who died from peripneumonia, occasioned by an osseous substance, the fourth of an inch thick, six inches long and three broad, extending under the third, fourth, fifth and sixth ribs of the right side. On the same side, towards the back, another bony substance was found connected to the bodies of the vertebræ and ribs. The patient had been troubled, for many years, with cough and difficult respiration, arising from the exostoses, which finally caused so much irritation as to destroy him.—Mr. Cooper mentions the case of a lady who had a very large and painful exostosis seated on the ribs, behind the right breast. The patient was almost exhausted when he saw her, and died soon after; but no opportunity of dissecting the tumour occurred.

Very considerable ossified depositions are found, occasionally, upon different parts of the vertebral column. In some instances, bony flakes supply the place of the ligament covering the intervertebral substance—In others, the intervertebral substance itself is converted into bone, rendering the spine a solid inflexible mass. I have seen a preparation, exhibiting the complete union of every vertebræ of the body, and constituting a solid pillar of bone from the head to the sacrum. In my collection, there are two specimens of several dorsal and lumbar vertebræ completely united by bone, instead of ligament; and the Wistar museum contains a most beautiful preparation of caries in the bodies of ten dorsal vertebræ, with destruction of the intervening cartilages, where

* Philosophical Transactions, vol. 8. p. 505.

a splint half an inch thick and twelve inches long, covered with protuberances, has been thrown out on the anterior and right side of the spongy bodies, uniting them closely to each other, preventing deformity and effecting a cure by firmly supporting the whole spine. Part of the spine of a horse belongs to the same collection, having the bodies of the dorsal vertebræ strengthened in a similar manner, by a bony splint, still more extensive. Freke has recorded the case of a boy whose back and ribs were covered with bony projections, from the neck to the sacrum. "April 14th, 1736," says he, "there came a boy of a healthy look and about fourteen years of age to ask us at the hospital, what should be done to cure him of many large swellings on his back, which began about three years since and have continued to grow as large on many parts as a penny loaf, particularly on the left side. They arise from all the vertebræ of the neck and reach down to the os sacrum; they likewise arise from every rib of his body, and joining together in all parts of his back, as the ramifications of coral do, they make, as it were, a fixed bony pair of bodice."*

All the bones of the pelvis are more or less subject to exostosis. The ilia, ischia, pubes and acetabula of a pelvis in the Wistar museum, are studded with tuberculated exostoses. They are particularly large and conspicuous, at the superior and inferior spinous processes of the ilium, and on each side of the outer surface of the pubis, over which thick bony masses, an inch long, project and unite with each other in a vaulted form, at the symphysis. Several bony plates, resembling the heads of large nails, are fixed upon each sacro-iliac junction, apparently serving the purpose of connecting the iliac bones firmly with the sacrum.—An exostosis upon the inside of the pubis has occasioned death, by compressing the neck of the bladder and preventing the introduction of the catheter.†—The *uterus*, has been ruptured by an exostosis, seated behind the pubis. A case of this kind occurred, several years ago, in the practice

* Philosophical Transactions, vol. ii. p. 252.

† See Boyer on the Bones, vol. 1st. p. 354.

of Dr. Dewees.* The patient died, and upon examination by Drs. Physick and Dewees, "the pelvis was found faulty at the superior part, by a *projection of bone or an exostosis*; it was situated a little to the left of the symphysis pubis, and looked towards the base of the sacrum; it diminished the superior cavity of the pelvis in the direction of its small diameter about half an inch; it was sharp and pointed at its extremity. The projection of the bony process just mentioned, by diminishing the cavity of the pelvis, must be regarded as the remote cause of this accident. The child's head, enveloped in a portion of the uterus, rested against this point so long, that inflammation and gangrene were the consequence. The efforts of the uterus continuing violent, and perhaps that violence increased by the difficulties which opposed them, eventually gave way, as its substance at this particular part was much weakened by the changes produced on it by inflammation; nay, the bony tumour acted somewhat like a cutting instrument."—Although I am unacquainted with a case of the kind, it is easy to conceive that an exostosis occupying the cavity of the pelvis, even when it does not attain considerable bulk, may present great difficulties to the passage of the child during parturition. Portal seems to have entertained the same view when he says, "Indépendamment des vices du bassin provenant de sa conformation vicieuse, sa cavité en général et ses deux détroits en particulier pourroient être rétrécis par des exostoses plus ou moins grosses; il peut aussi se former des tumeurs de diverse nature dans le bassin même, dans la matrice ou dans les parties molles adjacentes, qui empêchent la sortie de l'enfant de cette cavité."† Sarcomatous and encysted tumours within the pelvis are by no means uncommon, and have proved so injurious by interrupting delivery, that out of eighteen cases recorded by different writers, it appears, one half of the patients died and the greater part of the rest recovered imperfectly.‡

* Coxe's Museum, vol. li. p. 411.

† Cours d'Anatomie Médicinale, tom. i. p. 374.

‡ See Merriman's cases of tumours within the pelvis, in *Medico-Chirurg. Transact.* vol. x. p. 50.

Exostosis may occupy any portion of the thigh bone, but is seldom found either at its head or condyles. Every variety of the disease is occasionally produced. A femur in my possession is studded at different points, chiefly on the posterior surface, with numerous tuberculated exostoses of considerable size. There is, also, upon the same bone a circumscribed exostosis, four inches long; commencing below the great trochanter and bulging an inch beyond the shaft of the bone.—A very fine specimen of lamellated exostosis is contained in the Wistar museum. The femur is of extraordinary thickness (measuring in circumference eleven inches) uncommonly heavy, and covered with immense scales or osseous incrustations folded across each other and irregularly plaited like the skin of a rhinoceros. These masses extend from the trochanter to the condyles, and in some places are upwards of two inches long. The head and neck of the femur and condyles are of the natural size and free from disease. (Plate II. fig. 2.) Thirteen examples of exostosis of the femur are given by Daubenton,* in three of which the whole circumference of the bone is enlarged to the extent of nine and a half inches.—The work of Sandifort contains several tuberculated and circumscribed exostoses. Upon one bone a smooth tumour, four inches long, two thick, and shaped like a kidney, rises by a broad base, from the inner side of the thigh bone, near the lesser trochanter. In another specimen an exostosis of the same kind extends beyond seven inches; and in several others the tuberculated variety is distinctly marked.† Mr. Howship‡ mentions an exostosis, belonging to the collection at St. Bartholomew's Hospital, of such astonishing magnitude as nearly to equal three feet in circumference and conceal the whole length of the femur. The spinous exostosis is rarely met with on the femur; at least I know of three examples only—two in the Wistar cabinet, and one described and delineated by Cooper. In the former, each

* Description du cabinet du Roi, tom. 3d. p. 87.

† Museum Anatomicum, vol. 2.

‡ Medico Chirurg. Transact. vol. 8. p. 97.

thigh bone, just above the condyles, is irregularly enlarged twice beyond its natural size; and from this protuberance several bony spires project. The longest grow from the inner side, run upwards and parallel with the shaft of the bone. (Plate II. fig. 3.) In the case detailed by Cooper an exostosis, from one to three inches long, occupied the thigh bone above the internal condyle, and was felt through the integuments and muscles about the size of the finger. When the patient walked he felt what he described as a snapping in the part like a cord slipping from a pulley, which probably arose from the extension of the sartorius muscle and its sudden slipping over the swelling. When he placed the limb quite straight he found a difficulty in bending it; and when bent it was almost equally difficult to extend it: each flexion and extension producing a snapping noise, which could be distinctly heard.*

The tibia is extremely subject to exostosis. Among several specimens in the Wistar cabinet, four, particularly, deserve notice. In the first the bone is enlarged irregularly from the tubercle to the middle of the leg, three times beyond its natural size. The fibula also is increased in the same proportion and ankylosed with the tibia.—In the second, a circumscribed exostosis, the size of an egg, springs both from the tibia and fibula, about two inches above the ankle, firmly uniting, and at the same time forcing these bones out of their natural position.—In the third, the superior and inferior extremities of the tibia and fibula are enlarged and ankylosed; the fibula, especially, including two thirds of its superior extremity, is covered with very rough processes, and the bone nearly equals in size the tibia itself. In the fourth specimen, numerous spinous exostoses arise from each extremity of the tibia. Those attached to the upper end of the bone run downwards, while those seated near the ankle extend upwards nearly in a line with the tibia. Each extremity of the tibia is enlarged, as in the specimens of the same disease in the femur and humerus already described. (Plate II. fig. 4.) Many examples of exostosis

* Surgical Essays, p. 219.

of the tibia are found in different authors. Chesselden* has given a very fine engraving of a circumscribed exostosis, seated on the upper end of the tibia,—equal in size to a child's head. Sixteen specimens, varying in shape and size, are described by Daubenton.† The Museum of Mr. Charles Bell of London, contains numerous specimens of the same kind.‡ Several of the inferior animals are subject to exostosis of the tibia and metatarsal bones. The diseases called *splent* and *spavin*, so common among horses, are of this description. In my collection are two very beautiful preparations of the knee joint, of the horse, affected with spavin, in which bony matter has been thrown out profusely, so as to resemble the tuberculated exostosis in the human subject.

The upper and lower extremities of the fibula are more subject to exostosis than the intermediate portions. A fibula belonging to the Wistar Museum is represented in plate II. fig. 5., upon the upper end of which an irregular exostosis, as large as an egg, is seated. Each end of the fibula, belonging to the opposite leg, terminates in a bulb of considerable magnitude. In two fibulæ, in my possession, irregular exostoses cover the greater part of the bone, rendering each nearly as large as the tibia.

I have never seen true exostosis, either of the tarsal or metatarsal bones, and believe the disease to be equally uncommon in the toes. Two exostoses, projecting from under the nail of the great toe, are mentioned by Cooper, who removed one with a saw, on account of the pain it occasioned the patient. The same writer states that he has known half the foot amputated, on account of exostosis, at the extremities of the metatarsal bones next the toes.

Treatment of Exostosis.

In every true exostosis or simple enlargement from inordinate deposition of ossific matter, much may be done both

* Osteographia, Plate I.III. p. 53.

† Histoire Naturelle du Cabinet du Roi, p. 93.

‡ Description of the Anatomical Museum of the School of Great Windmill Street, 4to. p. 7.

by constitutional and local means. General remedies, especially when combined with rigid abstinence, exert a more powerful influence over local diseases than is commonly imagined. There is scarcely a tumour, however large or extensively involved, whether situated among vital parts or confined to the common textures of the body, which may not by judicious management, be made to feel the operations of the absorbent system. By abstracting from the body its accustomed nutriment, the proportion to particular parts is diminished and regular demands are then made upon all those textures which act subserviently in the animal economy. Superfluous fat is removed from its cells and made to contribute to the support of the general system. In like manner, unnatural enlargements and morbid superstructures are changed or extinguished and forced to renovate the waste which is perpetually taking place. A knowledge of these powers leads, in the hands of judicious practitioners, to very important results—such as are too often overlooked. The active aid of operative Surgery, instead of being regarded with horror and as an exercise of skill often ostentatious and unnecessary, is frequently employed before general remedies are even tried, and resorted to under circumstances where benefit cannot be expected to result. Whoever attends an European Infirmary will be struck with the diversity and multiplicity of operations, performed upon the most trivial occasions. Will it be credited when I say that the illustrious founder of American Surgery, during a practice, both hospital and private, of thirty years, and more extended than that of any other individual of our country, has hardly ever had occasion to resort to amputation? It may be asked what became of his patients? I answer, they have been cured by general and local means. Doctor, said a late Surgeon of the Pennsylvania Hospital, to a distinguished practitioner of a neighbouring city, what would be done in your town with such a leg? We should cut it off, was the reply. We can cure it *here* without, rejoined the other. “In a certain London Hospital,” says Dr. Gregory, “a patient was under the care of the physicians on account of a very bad

leg, which baffled their skill and appeared to them almost hopeless; they therefore requested a consultation of the Surgeons, to examine the leg and to decide what should be done with it. The Surgeons accordingly met, examined it, consulted about it, and resolved *nemine contradicente*, that the leg could not be saved and ought to be cut off. They cut it off without delay. But, strange to tell, the physicians, at their next visit, on examining the patient, found to their great astonishment, the supposed hopeless leg, as fast to his body as ever it had been. The puzzle was soon explained. It happened that the man had *two* legs, both of them very bad; one of them the physicians thought they could save, the other they despaired of. There being but a right and a wrong, it was not very marvellous that the consultation took the wrong. Both physicians and surgeons I believe were a little disconcerted at that *quid pro quo*; and as it was thought rather a strong measure to cut off both the man's legs, they exerted themselves to the utmost and saved the leg which should have been cut off: so that after all the poor man was but one leg out of pocket. As I was not an eye witness of this edifying transaction, it is proper to give some notion of the genealogy of the story, which is very short and simple. I have it from a reverend Clergyman, who had it from one of the physicians concerned, and who is now one of the most eminent of his profession in London. I know both the clergyman and the physician intimately; I know them both to be men of veracity and men of sense; and I have no doubt that the facts were just as I have stated them."* These remarks may appear, perhaps, irrelative, but I am satisfied of their importance, and shall be gratified if they tend to check the disposition, so prevalent among young surgeons, to use the knife; or contribute in a single instance to alleviate pain, or remove disease by milder or more appropriate means.

Wherever an exostosis be situated, provided it be painful, grows rapidly, presses on some important organ, impedes the motion of a joint or interferes with the flexion

* Additional Memorial to the managers of the Royal Infirmary, p. 265.

or extension of a limb, it will become expedient to interrupt its progress or remove it entirely. For this purpose we commence by gradually abstracting from the patient his accustomed quantity of aliment; by prohibiting nutritive articles, whether solid or fluid; by obliging him to avoid spirituous, fermented, or stimulating liquors of every description, and by restricting him to diet in every respect adapted to his age, constitution, and habits. It is impossible to lay down precise rules applicable to every case, since almost every individual is liable to have the natural actions of his body disturbed by causes which have no influence over the system of another. Many patients can live for weeks or months, on a little tea and a few crackers a-day, without being as much debilitated as others who are allowed animal food and more nutritive drinks. Much therefore will depend upon the peculiar constitution of the patient, and this may generally be ascertained in a short time. It is a great mistake to suppose that old people cannot bear a system of abstinence, or that debility is necessarily associated with age. There are many persons extremely vigorous in advanced life, who can sustain with impunity, deprivations sufficient to destroy younger and more delicate patients. It is true, however, that the systems of most old people sooner and more suddenly suffer from diminution of accustomed food, than those of younger ones. For this we should be prepared, and ready to substitute nourishment, so soon as we perceive the system suddenly yield. By pursuing this plan, steadily and for a length of time, we shall often succeed in removing exostosis and other tumours of considerable magnitude, without the employment of other means; but in general, it will be necessary to administer purgatives, every third or fourth day, or at stated periods, and to draw blood either from the system or locally. Local bloodletting, if the tumour be inflamed or painful, will prove extremely serviceable. Leeches should be preferred to cupping, and ten or twenty will be sufficient to detract several ounces of blood. They should not be applied over the most prominent or inflamed part of the tumour, lest ulceration

follow their bites.—Besides these remedies, pressure, gradually and steadily applied, will act powerfully, in promoting absorption. When the exostosis is seated upon the extremities, a firm compress supported by a roller will answer every purpose; but in certain situations, as on the cheek bones, it will be necessary to use a species of truss or an elastic spring, so constructed as to keep up moderate but constant pressure. Some tumours, however, will not bear even the slightest pressure, and if the patient is resolute enough to endure the pain, ulceration will be apt to follow—with increase of the disease. Sometimes pressure with adhesive straps, covered with sheet lead and a roller, answers a useful purpose.

Blisters, kept open by savin cerate, have been extolled by some writers as efficacious in exostosis; but I have tried them in two or three instances without effect. In venereal exostosis or node, they are sometimes beneficial.

When these remedies fail, and the exostosis continues to increase, becomes more painful, interferes with some important organ, or endangers the patient's life, an operation may, perhaps, be necessary. It cannot, however, always be done. Anatomical considerations will sometimes prohibit it—at other times, owing to the duration and size of the tumour, so much irritation would be produced as either to destroy the patient or render the cure very tedious or doubtful. If the tumour is not very large, is of the circumscribed, tuberculated, or spinous varieties, and situated superficially, such incisions should be made as to expose its base to the action of Hey's saw, or the trephine. By the conjoint operation of these instruments I removed, several years ago, from the tibia of a boy, in the Baltimore hospital, a circumscribed exostosis of considerable magnitude, which had resisted every other remedy. About the same period, I removed with success, and by similar means, a large exostosis from the outer condyle of the humerus, of a young gentleman, a patient of Dr. Allender, of Fell's Point. But often the exostosis is so situated, that neither the trephine nor Hey's saw can act without injuring, materially, the surrounding soft parts.

To obviate this inconvenience, several cutting instruments, within a few years have been invented. Mr. Machell* an English surgeon, has constructed a circular saw calculated to work beyond its semidiameter and at a considerable depth without interfering with the muscles. The saw is actuated by several wheels, inclosed in a case or conveyance, which are propelled by a winch or windlass. A pair of forceps are attached to the instrument, in order to fix it to the bone and keep it steady while the saw is in motion. Having never seen this instrument I cannot speak of it from experience; but if a proper estimate can be formed from the drawing and description, it would appear exceedingly complicated and expensive, and not calculated to answer, without great trouble, the purposes for which it was designed. The teeth of the saw, moreover, being placed on wheel-teeth or cogs, which work upon each other, must necessarily be injured or broken after a few revolutions. Again—unless the forceps be screwed with great force so as to take a very firm hold of the bone, the saw cannot be made to work steadily, and more or less resiliency or titter must follow every attempt to set it in motion. Both hands of the surgeon being employed, one in moving the handle, the other in holding the conveyance, it will appear evident that without the aid of the forceps the instrument, if it can operate at all, must do so under every disadvantage. It was on this account perhaps, that Mr. Cooper failed in removing an exostosis from the femur with this machine, until he secured it by the forceps. “The saw was then attempted to be used,” says he, “without the forceps, but it could not be well fixed: the forceps were therefore added to it, and the bone was sawn through.” But there are many situations where these forceps cannot be applied, or if applied, cannot render the saw steady.—The circular saw of Mr. Griffith† although extremely simple in construction, possesses so little power as to render it unfit for the most common purposes, and is decidedly inferior to that of

* Edinburgh Medical and Surgical Journal, vol. 11th. p. 273.

† Surgical Essays, p. 218.

Hey.—The chain saw of Dr. Jeffray* of Glasgow, appears to me calculated for no other purpose than that for which it was originally designed—the removal of carious joints; but under any circumstances it is extremely liable to twist or break; as I have several times proved in operations upon the dead subject. The cutting plyers of Griffith and Liston are fit only to detach small spicula of bone or to remove loose carious portions.†—To overcome difficulties insurmountable with Hey's saw, the trephine, &c., I suggested to Mr. Adam Stewart, (an ingenious mechanician of this city) the idea of constructing an instrument calculated to work at considerable depth, without injuring the soft parts, and propelled by a spring of sufficient strength to create several hundred revolutions of a circular saw within a few seconds. The sketch, (Plate III.) represents a machine of this description, exceedingly simple, yet so powerful as to divide, with the utmost celerity, the hardest bone or ivory; making a cut proportioned to the size of the saw, and the pressure with which it is applied to the part. The superior extremity, or expanded portion of the instrument, incloses, in a brass frame, a strong clock spring, attached to a wheel, and from this a connecting rod extends to the pinion of the saw and gives it motion. The spring is wound up by a winch and restrained by a catch, until the operator wishes to set it free; which he does by slight pressure with his thumb or finger on the catch. The instrument may be held in the hand, like a writing pen, and the saw directed to any part we desire to cut; its velocity being regulated or its movement instantly stopped, if necessary, by the catch. Several saws of different sizes are adapted to an axle placed at right angles with the shaft of the instrument, and by means of a small spring can be instantly changed. The axles, also, are of different lengths, so as to extend into the deepest wound or to work near the surface, as may be required. To prevent the saw from being stopped at its

* Jeffray's observations on the excision of carious joints, p. 175.

† See Edinburgh Journal, No. LXXI. January, 1821, p. 155.

semidiameter, cutting edges or teeth are given to the axle, which enable it to make its way through the bone, so as to divide it perpendicularly while the saw itself moves in a horizontal direction. By this contrivance very large exostoses may be removed at once, or by repeated applications of the instrument, which could not be reached in any other way. Although I have yet had no opportunity of trying this saw on the living subject, I have made so many experiments with it on the dead, as to be perfectly convinced of its power and utility. It is adapted to many purposes besides the removal of exostosis, and is admirably calculated for splitting a bone, to any extent; cutting out a portion of it and removing sequestra in cases of necrosis, &c. In such cases a plain axle, instead of a toothed, should be used. The only case in which I should hesitate to employ it would be on a fractured skull; in exostosis of the cranium, however, it may be employed with as much safety as any other instrument. In case of necessity a forceps may be adapted to the instrument and the spring rendered quiescent, so as to obtain by the use of the handle, the exact principle (but in greater perfection) of Machell's saw.

Mr. Cooper has given the history of five operations for the removal of exostosis. The first was of large size and formed like an inverted pyramid, the apex of which was attached to the tibia; it had been growing forty-four years. An elliptic incision was made through the integuments, and the tumour removed by the amputating saw, without much difficulty. In the second case, a spinous exostosis was taken from near the inner condyle of the thigh bone of a young lady eleven years old. The limb had been painful as low as the heel, and when the patient "attempted to run, she felt a snap upon the swelling, as if a cord had slipped out of its pulley, which was owing to the tendon gliding over the projecting part of the bone." The tumour was separated by Machell's saw, and the irregularities that remained smoothed by the bone nippers.—The circumstances of the third case were nearly similar to those just detailed.—In the fourth case, an exostosis of the fibula,

involving the fibular nerve, thereby producing paralysis of the peroneal muscles, as well as the flexors of the foot and extensors of the toes, was removed by Hey's saw. In the fifth case, a large exostosis, growing from the outer and left side of the pubis, attended with great pain in the hip, above and below the knee, and in the foot, was separated by Machell's and Hey's saws. These patients were all cured except the fourth, who was relieved of pain, but not of the paralysis.

In 1815, a gentleman, from the Eastern shore of Maryland, applied to me on account of a circumscribed exostosis which occupied the middle of the os frontis, had been growing for years, arose without evident cause, attained the size of a large egg and was solid and incompressible. Ulceration possessed the most prominent part of the tumour, which was supposed by the patient to be cancerous. As various remedies had long been tried, ineffectually, an operation appeared to furnish the only prospect of relief. An incision, traversing the whole swelling, was made, and the integuments turned aside, so as to expose the base of the exostosis, which was then removed, with difficulty, by Hey's saws. The substance was reticulated and did not differ, apparently, from that of sound bone. The internal table of the skull remained entire, the patient recovered in four weeks and the complaint has never returned.

An exostosis may be removed from almost any bone of the body; provided it does not embrace its whole circumference, or extend throughout its length—as in the lamellated exostosis represented in Plate II. fig. 2.—When a very large bony tumour surrounds the arm, forearm, thigh or leg, and an operation becomes necessary, nothing less than amputation will probably answer.—There are certain situations, where it would be difficult, if not impossible, to remove even a circumscribed exostosis occupying part of a bone. Thus such a tumour growing from the inside of the pubis or sacrum, interrupting the flow of urine, or impeding the passage of the child in parturition, could not, perhaps, be reached or extracted. In the latter case, how-

ever, so much danger, probably, would not result, as an experienced accoucheur would attempt to turn the child and avoid the projection.—The removal of an exostosis arising from the inner surface of the ribs, as in the case related by Giffard, to which I have already referred, would be attended with great difficulties. The same may be said of the disease when seated on the spine, except the spinous processes alone should be involved; in which case an operation might be very beneficial; as shall be shown hereafter, when I speak of an osteo-sarcomatous tumour, of monstrous size, which I removed, in October, 1818, from the spine of a young gentleman in Maryland.—An exostosis, growing from the inner table of the skull, and pressing upon the brain, might perhaps be removed;—but not with much prospect of success. This would arise less from the difficulty of the operation itself, than from the exposure and subsequent inflammation of the brain and its membranes. Cases may possibly present, however, where the surgeon would be justified in resorting to an operation;—as in the examples recorded by Howship* and Wadd,† of ossific matter deposited between the tables of the skull, distorting the bones and producing insanity by pressure on the brain. The successful issue of the interesting operation, by Keate‡—the removal of a bony tumour involving the lamellæ of the greater part of the os frontis and containing an enormous collection of hydatids, should form an additional inducement to saw away any bony mass encroaching upon or interfering with the functions of the brain.—An exostosis of the lower jaw, involving its circumference, might possibly be removed by cutting through the bone on each side of the tumour and completely insulating it. The void would afterwards be filled by callus, or the cut extremities of the bone so firmly united by ligamentous substance as to render the jaw nearly as useful as ever. Again—if the tumour should be situated at the symphysis, the jaw could

* *Medico Chirurgical Transactions*, vol. 8th.

† *Malformation, and diseases of the head*, by Wm. Wadd. 4to. 1819.

‡ *Med. Chir. Trans.* vol. 10th.

readily be cut through on each side of it, and the ends of the bone pressed together and united, as in Dupuytren's celebrated operation for artificial chin.* Great deformity, however, must necessarily result from such a proceeding, and in many instances I think the operation would be totally unnecessary, and should be supplied by the following mode, which I adopted, three or four years ago, in a case at the Baltimore Alms House. A poor woman had a tumour partly fungous, partly bony, the size of a large orange, which sprang from the symphysis of the lower jaw, on the inner side, pushed before it several teeth, and so filled the mouth as nearly to prevent swallowing and render articulation almost unintelligible. In presence of Drs. Caldwell, Bell, Hayden and others, I divided, with a strong curved knife, the alveolar processes, (softened by caries) on each side of the symphysis, nearly to the base of the jaw, separated the tumour from its attachments, and controlled the hemorrhage by ligatures and the actual cautery. Upon examining the cavity or bed occupied by the tumour, I was surprised to find how much of the jaw had been absorbed—a small portion only of its base remaining on each side of the symphysis. Great as the vacuum was, however, it was soon filled by granulations and ossific depositions, and in a few weeks the patient entirely recovered. Nothing could have been easier, in this case, than to have imitated Dupuytren, by cutting away the base of the jaw and bringing together its ends; but if I had done so the patient would not only have been horribly disfigured, but mastication and speech greatly injured by the teeth and tongue being thrown from their natural situation.

Should an exostosis occupy the orbit and displace the eye, it may be removed, provided the base be not very large; not by following the practice of Lucas†—cutting down upon the tumour and then leaving it to drop off by caries—but by applying cautiously Hey's, or a small circular saw as near to its base as possible. If the tumour cannot be reached by a saw, it may, perhaps, be made to exfoliate by

* See *Diet. des Sciences Medicales*, tom. xxix. p. 430. † See p. 125.

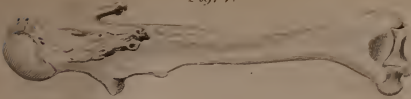
the application of acids or caustics. An interesting case illustrative of the beneficial effects of caustic, is related by Louis.* A woman, thirty years of age, suffered from a fistula lacrymalis which resisted every remedy. The bones became enlarged, and an exostosis, the size of an egg, occupied the os planum and internal angular process of the os frontis. The eye was forced from its socket and hung over the cheek. Brissant, the surgeon who attended the patient, applied caustic to the exostosis; suppuration followed, and in three or four months the greater part of the tumour exfoliated, the eye resumed its position, and a cure was soon completed.—Neither the saw nor caustic, however, will often be required for exostosis of the orbit, as the disease must be considered rare. Tumours of a different description—steatomatous, encysted and sarcomatous, frequently occur, and become so large as to displace the eye. Several such cases I have seen. I know a gentleman at the present time, whose eye is pushed to one side and almost squeezed from the socket by an encysted tumour; which could be removed, with success, and with the utmost facility. In the year 1818, I took from the orbit of a patient, under care of Dr. Coulter, of Baltimore, a sarcomatous tumour, which grew from the socket, compressed the eye, and in time, would have destroyed it. Similar examples are related by Trincavelli, Paau, Astruc, Bertrandi, Bonetus, Louis, Le Dran, St. Ives, Hope, Broclesby, Rodman,† &c.

There is another operation for the removal of exostosis, which deserves to be tried, whenever the situation of the disease will admit of it, before we resort to extirpation with the saw, or to amputation of a limb. This consists in the separation of the periosteum from the tumour—thereby, in a measure, depriving the part of vascularity and promoting its absorption. The remedy was proposed some time since,

* *Memoires de l'Académie de Chirurgie*, tome xiii. p. 277.

† See Schenkius—*Memoires de l'Académie—Sepulchretum Anatomicum—Maladies des yeux—Philosophical Transactions*, 1744—*Medical and Physical Journal—London Med. Obs. and Enq.*

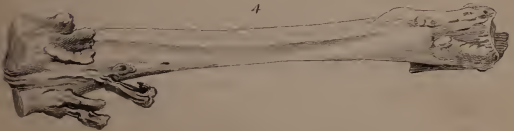
Fig. 1.



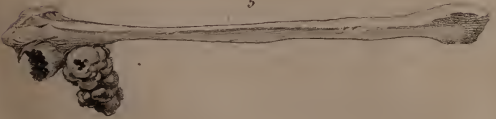
3



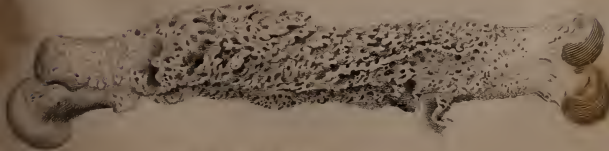
4



5

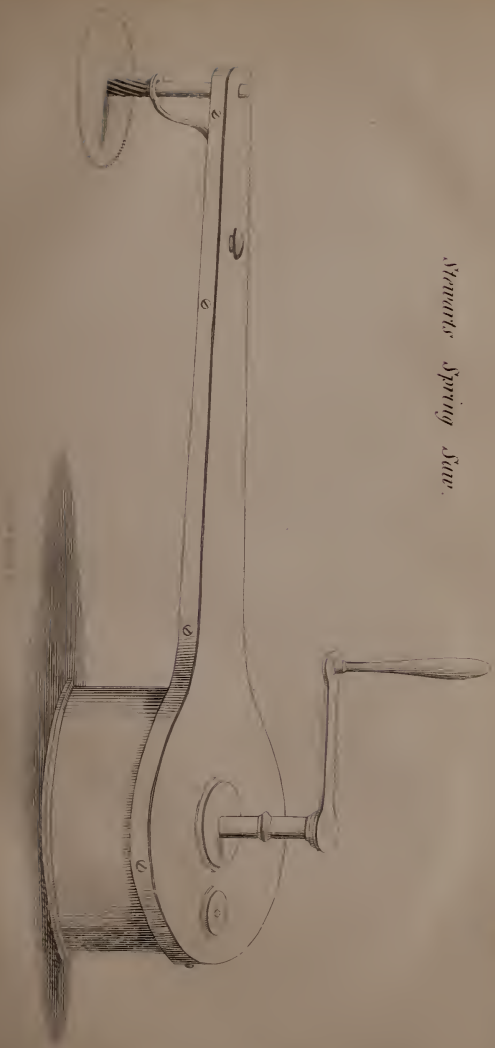


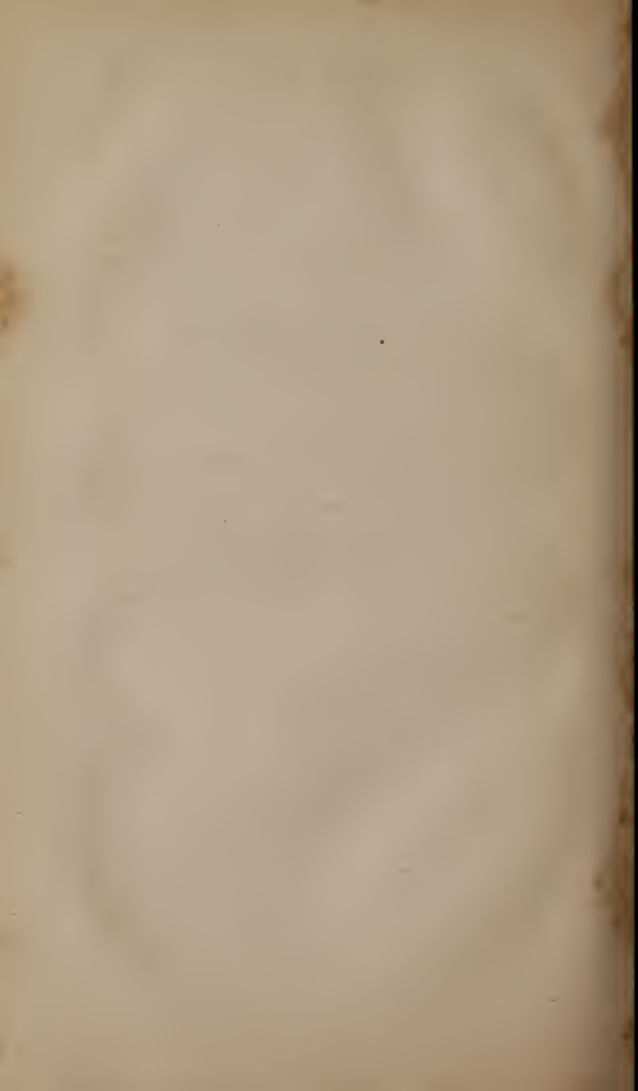
2





Stewart's Spring Saw.





by Mr. Astley Cooper, and communicated to me in a letter, by Mr. Charles Bell, of London. "Mr. A. Cooper told me," says Mr. Bell, "last night at the Anatomical Society, that he finds the largest tumours of exostosis, absorbed by simply cutting down upon them and separating the periosteum from the face of the bone by the handle of the knife." I have not had an opportunity to test this practice, and therefore cannot speak from experience ; but I am inclined to believe that it will not answer the high expectations we should naturally form from the commendation of so distinguished a surgeon as Mr. Cooper. I am induced to draw this conclusion, from knowing that the older surgeons were in the habit of performing the same operation in cases of venereal exostosis or node, sometimes with success, but frequently without any benefit whatever.

Of the venereal exostosis I might next speak, but as the disease differs essentially from common exostosis and requires a distinct mode of treatment, it should be considered under a separate head. In a subsequent number of the *Journal*, I propose to make some observations on osteo-sarcoma and spina-ventosa, diseases often confounded with exostosis, but in reality, bearing to it only a remote resemblance.

CASES.

ART. VIII. *An Account of a Lusus Naturæ.* By JOHN LIVINGSTONE, Surgeon of the British Factory at Canton. Communicated by J. R. Mitchell, M. D. &c.

IN Europe monsters never fail to excite much public attention; they readily obtain a place in Museums and the cabinets of the curious; and even slight deviations from nature, such as a finger or a toe, more or less, have been the subject of elaborate memoirs, perhaps in many respects disproportionate to their real importance. In China the case is quite otherwise. We know of no such collections. I understand from Dr. Morrison that their books are silent on this subject, and the very extraordinary and interesting monster, which I am about to describe, was born only two days journey from Canton, about sixteen years ago; has been exhibited at Canton, and all around ever since; yet as far as my inquiries have extended, no account of this *Lusus NATURÆ* has hitherto been drawn up, or has come to the knowledge of any European.

When I was informed, that a monster was to be seen in a temporary enclosure near St. Agostinho's church, Macao, I lost no time in attempting to gratify my curiosity; but I learned that the monster was then unwell, and had retired to rest. I then formed the resolution of having him brought to my house, for the double purpose of more deliberate observation, and having at the same time a correct model made under my own eye; but aware that the only good artist then in Macao was employed, I deferred giving my orders for a few days; in the mean time, the monster unexpectedly left Macao.

However the modeller had made such careful observations of the subject, that he informed me he could make an exact representation of what he saw. He has succeeded so well, that I am assured by many friends who had carefully

examined the original, that the model is wonderfully exact:—a few unimportant exceptions shall be pointed out in the order of my description. I have spared no pains in collecting information from every quarter. I have had the advantage of receiving accounts from a great many intelligent friends, among whom I have the pleasure to mention three medical gentlemen of this place. All their accounts agree surprisingly well. The model has been shown to many of them, and my account read, with both of which they are entirely satisfied—so I am persuaded that my own observations could not have added much either to the value or variety of those which I have been so fortunate as to receive from others.

A-ke was born sixteen years ago, in the district of Yunlang-yuen, with another male child of nearly the same size united to the pit of his stomach by the neck, as if his brother had plunged its head into his breast. The skin of the principal here joins that of the upper part of the neck of the parasite, quite regularly and smoothly, excepting the superficial blood vessels, which appear somewhat turgid. The sufferings of the mother were so great that she survived the birth of this monster only two days.

Since that time, the parasite has not much increased in size,* and at present is not much larger than new born infants usually are; but the bones are completely formed. The shoulder bones are remarkably prominent. Here the model is faulty, since it represents the roundness of infancy, but all this plumpness has disappeared from the original, where bones seem only to be covered with skin. The hips of the model are too prominent. The manner in which

* I have the authority of lieutenant-general Wood, for stating that a careful admeasurement of the parasite was made at his request; the trunk and neck measured about eleven inches, and the longest limb thirteen inches, making the extreme length two feet. This accords sufficiently well with the size I have mentioned, but as the modellers in China do not work by any scale, it would be useless to deduce any *exact* measurement of the whole figure by knowing a part.

.* General Wood is still at Macao waiting a passage for England. The general had A-ke at his own house, and examined him with great attention. He saw the pair urinate simultaneously—but had no opportunity of witnessing the consensual erection and *stillicidium* mentioned by Dr. L.—. The India Company's printer at Macao, saw A-ke often, and has sent his statement concerning him to England.

J. R. M.

the thighs appear is quite happy, but the feet, particularly the left, are not sufficiently clubbed. In the original, generally the feet and toes are less perfect than in the model. The toes adhere, and one or two are wanting.

The attachment of the neck of the parasite to the chest of the principal, admits of a semirotatory motion. The natural position of the bellies is towards each other; but A-ke can turn his brother so far round that he can bring either side towards his own belly. He also shows that his brother's arms can be moved freely. The thighs and legs remain stiffly bent, as represented in the model: the thigh being ankylosed with the *ossi innominata* above, and the *tibiæ* below. The genital organs appear too perfect in the model, since no vestige of testes, and very little scrotum, can be perceived in the original. The *penis* is however large in proportion, and the *glans* about half covered with the prepuce, and is subject to occasional erections, in which state a *stillicidium* of a mucous fluid from the *urethra* has sometimes appeared, and has induced a belief among the Chinese, that the seminal fluid is copiously secreted. The kidneys seem to perform their functions perfectly. The anus is wanting.

A-ke is now about four feet and ten inches high, of a feeble frame and sickly appearance: but excepting the encumbrance above described, he is in all respects perfectly formed. He appears to be sufficiently conversable and intelligent, and says he has the same feeling of pain, if any part of his brother's body is hurt, as if it was the same part of his own body: even the slightest touch which would be perceptible, if applied to his own person, is equally perceptible if applied to his brother. This statement was most satisfactorily confirmed by an ingenious medical gentleman, who, observing A-ke's attention to be fully employed, and his head turned away in a contrary direction, pinched quickly the hip of the parasite; A-ke instantly struck the same part of his own person, just as if that had been the pinched place.

Formerly he had reason to imagine, from certain ob-

scure motions which he perceived within his brother, when he was himself in pain, that all their feelings were reciprocal; but for some time past he has not been sensible of this; excepting when he has occasion to make water. His brother then never fails to void his urine at the same time, whether he has occasion to relieve nature, or to gratify the curiosity of spectators.

A-ke's respiration is never perfectly free, on the contrary, it is commonly laborious, and on the slightest exertion, such as walking to a little distance, ascending a flight of steps, or the like, he breathes quickly, and with difficulty. To relieve this he supports the parasite with his hands, but to obtain any considerable degree of ease, a recumbent posture is necessary. His pulse is commonly quick and small. Mr. Gomez felt distinctly the pulsation of the carotids in the neck of the parasite; it was feeble. He also examined carefully the pulse at the wrists, it was very slow (*valde lente*.)

The usual temperature of both is natural. A-ke wears an unusual quantity of clothes, yet he never appears to perspire even in the warmest weather. His usual gait is unsteady and feeble: when he walks up or down stairs, he supports himself with one hand, and his brother with the other, and brings both his feet upon the same step, before he attempts to advance another foot.

When in his best state of health, he informed Mr. Gomez his appetite was so good that he could take as much food as any three of his age; at present his health in general is much impaired. He complains of weakness of stomach, loss of appetite, defective and painful digestion; so it is commonly thought that he cannot live long. His countenance is sallow and more emaciated than it appears in the model.

A-ke's father is one of the poorest class of husbandmen. He has been content to hire his son for five Spanish dollars a month to the man, who has for his trouble all the profits of the exhibition. Ten cash (less than a penny sterling) is the price of admittance into the enclosure, which they make

in public places. He walks to private houses; the parasite appearing while going through the streets like a tumour under his clothes. On these occasions the exhibiter is content to receive whatever is given. He commonly gets half a dollar, or a dollar. The concern does not appear to be profitable.

Having stated all the circumstances of this wonderful and most interesting case, as fully as they have come to my knowledge, I might be excused from making any observations,—the field is ample, and no doubt a variety of ingenious opinions will be formed. I think however you will be desirous to have my reflections on some points; I shall therefore mention a few.

It will probably be admitted, that as the quantity of nourishment which the parasite derives from the principal system, is only sufficient to preserve life without adding to the bulk of its parts, it receives blood only from small arteries, perhaps from the branches of the mammary arteries, where they freely inosculate with the large branches of the epigastrics, forming arteries which may either immediately anastomose with those of the parasite, and supply its veins and heart with blood, sufficient to support a species of circulation, similar to that of the *fœtus in utero*: the principal supplying the place of the *placenta*, or the blood may be returned to the principal, by a set of veins peculiar to the parasitic state of existence. It is highly probable that the entire pulmonary system is wanting, or in a state of complete torpor, and from the flaccid appearance of the abdomen, we can scarcely doubt but the *chylopoietic viscera* are in a similar state.

Considering the Chinese account of the seminal secretion as founded in error, the parasite can only be regarded as having the kidneys in an efficient state, besides the circulation of the blood, and absorbents. This state seems to admit of no other function.

This view of our subject accords sufficiently well with that Theory of Monstrous Productions which supposes that two distinct embryos had coalesced by some accidental

circumstance, which may have caused the amnions of each to adhere; and controverts an opinion which at one time had many advocates respecting the use of the *liquor amnii*. It may be conjectured on the same view, that the great sympathetic nerve of A-ke supplies the urinary and genital systems, and that the nerves of his skin are diffused over that of his brother also. All this will require that our notions of the nervous system shall be considerably modified before we can be enabled to account for the few, but decisive facts which belong to this part of our subject: to account for these uncommonly received principles, it will be necessary to suppose that the monster had the same conformation in the primordial germ. This conjecture removes some of our difficulties. It explains how the brain of A-ke is in all respects a *Sensorium commune* to both. That the parasite is therefore only a duplicate of the principal, not more difficult to be imagined than a supplementary finger or toe. Here however our field expands into a wilderness, into which it would be unsafe to enter without a guide.—I shall therefore resign the task into the hands of more adventurous discoverers.

ART. IX.—*Cases illustrative of the use of Mercurial Ointment in Erysipelas, Swelled Leg, &c. &c.* Communicated by P. W. LITTLE, M. D.

MUCH has been said of late of the efficacy of Mercurial ointment in Erysipelas, and the credit of introducing the practice, seems very generally to be accorded to Dr. Dean, of Chambersburg. But the fact is, it originated with me, by whom it was communicated to that gentleman, when on a visit to this place, in February, 1817, as can be fully proved.

I have hitherto been restrained from the assertion of my claims in this instance, by a wish to establish the utility of

the practice by further experience, and I can unaffectedly declare, no less, by a reluctance to appear before the public in the character of a writer.

CASE I.

In the winter of 1816, I was called to see a daughter of Captain Wilson, aged eight years, who had five weeks before, accidentally run a thorn between the big and next toe of the right foot. The thorn was extracted, and no bad effects ensued till a few days before my visit, when the wound began to inflame and to be painful. Cataplasms of bread and milk were applied by the direction of the mother till the swelling subsided and the sore seemed healed. Two or three days had only elapsed, however, when she complained of great lassitude, and much pain in the head and neck, with nausea and occasional vomiting. An ounce of castor oil was given by the mother, which operating freely afforded considerable relief. But the next morning the pain of the head and neck returned with increased vehemence, accompanied by a general soreness of the muscles of the body and extremities, and at noon she was seized with spasms, which were to an alarming degree, though of short duration. Early the following day, there was a recurrence of the spasms still more violent, and of longer continuance—I then was consulted in the case.

I found her with every symptom of confirmed tetanus, sometimes assuming the shape of opisthotonos, and at others, of emprosthotonos, and occasionally of trismus. After the operation of a cathartic, I opened and enlarged the wound which was healed, applied caustic to it, and subsequently an emolient poultice. The foot and ankle being swelled, and erysipelatous in appearance, I also covered these parts with a blister. But the disease continuing to advance, the inflammation extending up to the ilium, with appearances altogether unfavorable—though these and a variety of other remedies were tried, I resolved to bring about a salivation. To effect this purpose, I caused the strong mercurial ointment to be rubbed freely on the extremities,

as well as on the throat, morning and evening, and in the interval, had a saturated roller applied over the inflamed surfaces. As early as the third dressing, I perceived a manifest improvement in the swelling, and on the fourth day, the local affection was entirely well, and with it a complete removal of the general disease:

CASE II.

In 1820, I attended a female of sixty-five years of age, of a gross habit of body, labouring under a most extensive erysipelatous inflammation, which had located itself in the back. It originated from a small pimple, which appeared in the lumbar region, and from the pain and hardness, was considered for many days, as merely the beginning of a common bile. It continued to increase in size, and the hardness and redness to extend, with an intolerable itching and burning sensation. But it was not attended by any general derangement of the system, till three days prior to my visit, when I found her, with a low fever, exceedingly debilitated, and with all the indications of gastric disturbance.

The inflammation had extended from ileum to ileum, and above the points of the scapulæ. It was of a dark red, very hard, and excessively painful to the touch, having the cuticle in different places raised, underneath which the aspect was dark and gangrenous.

The treatment was commenced with a dose of salts, and by covering the inflamed parts with strong mercurial ointment, directing it also to be rubbed in, morning and night.

The next day she was evidently better. Continuing nearly the same treatment, alternately purging and using nitrous powders, with the mercurial dressings for five days longer, she was considered convalescent. But the part originally affected, sloughed to the extent of three inches, leaving a large cavity, over the spine, which gradually filled up. It is, however, worthy of remark, that in consequence of improperly indulging in stimulating food, the edges of the wound became hard and erysipelatous, which were removed by the reapplication of mercurial in place of basilicon ointment.

CASE III.

Nearly about the same period, I attended a female child of Mr. Chesnut, aged four years, with one of the worst attacks of erysipelas which I ever witnessed. Commencing on the back of the neck, it soon pervaded the whole body. The face was much swelled, the eyes closed, the person generally œdematous, in many parts of a livid hue, with ichorous vesications. Her pulse was weak, small, and quick, the tongue furred, the breath fœtid, and a heavy coma existed.

Entertaining the notion, that the disease was of gastric origin, I began the management of the case with an emetic, which was followed by active purging with calomel and jalap, and in the intervals, antimonial and cretaceous medicines were interposed.

The entire surface was dressed with strong mercurial ointment, though less was applied to the head, than elsewhere.

The first dressing proved obviously beneficial, by relieving the burning and itching, which harassed, distressed and rendered her restless, and extremely fretful during the evening exacerbations. The inflammation rapidly declined after the second dressing, and in a few more applications of the ointment she was well.

CASE IV.

On the 23d of Jan. 1817, Mrs. B., of a delicate constitution, was favourably delivered of her first child, and six days afterwards became ill of puerperal fever, from which, however, she soon recovered. On the 12th of Feb. I was again requested to see her, and found her labouring under that singular complaint called swelled leg,* attended by the usual symptoms of low fever.

Being exceedingly reduced by previous disease, and copious depletion, I was precluded from the use of the lancet, and after moderate evacuation of the bowels, I

* Phlegmasia dolens.

administered a combination of calomel and opium. Baffled in my attempts to cure the disease, by this, and many other measures, general and topical, without indeed producing even any very sensible alleviation, I determined to try the mercurial ointment, exactly as in the erysipelatous cases. To my great satisfaction, when I saw her four days afterwards, she was sitting up in bed, nursing her child, suffering no pain, the swelling nearly gone, and her condition in every way so much amended, as no longer to require my attendance.

P. S. Previous to the use of the ointment, I met with no disease more difficult to manage than erysipelas, in relation to topical remedies. But since, very little in this respect, governed as I am, in the general treatment, by the state of the system. I have found it beneficial in all the different species and grades of that disease, and from parity of reasoning, would suppose it an appropriate remedy to confluent small-pox, and some cases of measles. Twice I have prescribed the ointment mixed with charcoal, in gangrene, once of the scrotum, and in another instance of the foot, with decisive advantage. It excited a healthy action, threw off the sloughs, established the suppurative process, and destroyed the fetor. In carbuncle, I have witnessed very beneficial effects from it, and also in chilblains, and paronychia. Early applied in the latter affection it often prevents suppuration. Last winter, I was consulted in a case of extensive inflammation of the ear, which had suppurated and discharged very freely for weeks. The pain attending it, was most excruciating. After a time, the discharge ceased, and was followed by an extensive swelling back of the ear. Cataplasms of different kinds were applied for ten or twelve days, without reproducing the discharge, or effecting any mitigation of pain: on the contrary the swelling continued to increase in size and hardness. In this stage of the affection, I was consulted, and on examination found the tumor to be scrofulous, attended with erysipelatous inflammation, which was becoming gangrenous. I directed the ointment to be freely applied to the swelling,

and introduced into the ear. The good effects of the remedy were soon experienced, and with the aid of such medicines, as the case seemed to demand, an entire cure was readily accomplished.

NOTE BY THE EDITOR.

In the last two years, the mercurial ointment has been a good deal employed in erysipelatous inflammation by Dr. Physick, and some other distinguished practitioners in this city,—and as far as we have been able to learn, with much success. There is a form of the disease, not noticed by our correspondent, in which we have seen it exceedingly beneficial. The case to which we allude, makes its appearance very often in infants a few days after birth, and in its more violent attacks, breaks out and diffuses itself over the lower portion of the belly, descending between the thighs to the breech, involving the genital organs.

It differs from common erysipelas in this, among other respects, that it seems mostly to be seated in the cellular membrane, which becoming inflamed and thickened, produces a swelling hard and prominent. The appearance, at first is florid, though not uniformly. Cases we have now and then met with, where there was a great degree of intumescence, without any discoloration, resembling very closely phlegmasia dolens. It is most apt to assume this character, we think, in adults, or children some years old, and we have found it in every instance, located in the thigh. Dressings of mercurial ointment, aided by copious evacuations of the primæ viæ, will generally effect relief in this disease, which hitherto has proved exceedingly intractable to other remedies.

As regards the utility of the ointment, in the swelled leg of puerperal women, our own experience does not warrant us to express an opinion. We learn, however, from Dr. Dewees, whose opportunities of determining the point have been ample, that he does not repose much confidence in it. Thirty years ago he tells us, it was used by his preceptor Dr. Smith, and probably by other physicians of that period.

He has also in his own practice, tried it repeatedly. We shall be glad to hear further from our intelligent correspondent on this subject, and we will add for his information, that the ointment is not the only mercurial preparation of use in erysipelatous affections.

We have long known a solution of corrosive sublimate, as a popular remedy in inflammations of this nature, from poisons, and in the proportion of one grain to an ounce of water has been strongly recommended in genuine erysipelas, by Dr. Schott, an excellent practitioner of this place. Citron ointment we have for many years directed in one variety of erysipelas, that of the nose, symptomatic of disordered chylopoietic viscera, and a drachm or two of calomel to the ounce of lard, or equal portions of calomel and chalk dusted on the part, afford the most effectual means with which we are conversant, in the erysipelatous eruptions, incident to dentition.

ART. X. *A Case of Hydrocephalus*, communicated by JOSEPH GLOVER, M. D. Surgeon General of the State of South Carolina.

ON the 13th of December, 1817, I was requested to visit the child of Mr. H. on South Bay. I was informed on my arrival, that Mrs. H. had been delivered of a female child, on the 21st of the preceding month; that the birth was not premature; that she had been in good health during her pregnancy; that she had previously borne five healthful children, and that the child to which I had been called, was in every respect perfect, except that she had a watery head, which was discovered shortly after her birth; that various remedies had been tried without effect, and that her head had increased in size so much as to give great uneasiness to her parents, and was evidently very distressing to the child herself.

On examination, I found the head of the child much enlarged, being eighteen inches and a half in circumference,

measuring over the frontal and occipital bones; and nineteen inches and three fourths, measuring from under the chin over the ossa parietalia. I discovered all the proper bones of the cranium to be considerably separated at their sutures, and found an evident fluctuation of water within them.

The child was to appearance, (in every other respect,) healthful: had no symptom of fever, that I could discover, and was reported to me as nursing well. There was however an evident distortion of her eyes, and moreover she had a countenance from which nothing favourable could be drawn. I therefore deemed it my duty to inform her parents, that from my own experience, as well as from the cases of which I had read, little more than hope remained of the recovery of their child; suggested a plan of treatment and retired.

I repeated my visits at uninterrupted periods, until the second of March following, during which time, I attempted every plan of treatment for the relief of my patient, which either my judgment dictated, or that I could collect, from the authors on the subject, which were within my reach; but they were of no avail. The disease continued to increase with great rapidity, and nothing but a prolonged existence of misery, and an untimely death, appeared to await my patient, her head having now increased in size to two feet in circumference, measuring over the frontal and occipital bones, and one inch more measuring from under the chin over the ossa parietalia. Under these circumstances, I thought proper to advise the operation of tapping the head, as the only means left within my power, which appeared to afford the least prospect of relief to my patient. To this her parents readily consented. I therefore invited several of my medical friends, and performed the operation on the day following, being the third of March, 1818, in the presence of Dr. JOSEPH JOHNSON, Dr. WHITRIDGE, Dr. WARING, Dr. FROST, Dr. HUME, and my brother, Dr. H. C. GLOVER.

It was done in the following manner:—Perceiving that the fluctuation was most distinctly felt, on the right side of

the child's head, between the parietal and temporal bones in the course of the squamose suture, I punctured the head in that part, with a common lancet, and by introducing a grooved director into the incision, was enabled to let off the water as freely as I wished. A person who was not present, would perhaps suppose, that the danger of wounding a branch of the temporal artery, would render this a very improper place at which to puncture the head, but I can assure you, that I could discover its course so readily in this case, that it was not at all difficult to avoid it.

There was scarcely any discharge of blood from the puncture; but when we had drawn off a pint of water, it was perceived that the bones of the cranium were so slightly attached to each other at their sutures, and the collapse of the integuments was so great, that it was judged advisable to withdraw the director, and take away no more water at that time. The water drawn off was such as we generally see in ascites, and in dropsical depositions in other parts of the body.

In attempting to bandage the head, the bones yielded so much, after the water had been thus evacuated, that there appeared to be great danger of injuring the parts within the cranium, on lifting the head with our hands. I was therefore obliged to slip a wide bandage under the head, (with which to make the pressure more equal when the head was elevated,) before I was enabled to apply the necessary bandages for completing the dressing. I made no other application to the wound made with my lancet, than a plaster of oil and wax, over which I placed a compress and two circular bandages; (for the purpose of supporting the bones and bringing them in contact at their sutures) the one around the head over the frontal and occipital bones; the other, from under the chin and over the ossa parietalia. The child appeared to suffer very little pain from the operation, and when I visited her on the evening of that day, nothing had occurred worthy of being noted down.

On the morning of the fourth of March, I found my patient as well as usual. She had been somewhat restless, during

the first part of the preceding night, but was soon relieved by an anodyne, which (I have omitted to mention) her mother had been obliged to give her repeatedly before, to procure sleep. I was informed that she had passed an unusual quantity of water by urine since the operation.

It was my intention to have introduced my director at this visit, and to have let off a further quantity of water, before the lips of the wound had healed; but, the day being cold and rainy, I thought it most adviseable to defer it.

On the morning of the fifth of March, the weather being favourable, and my patient being as well as usual, (in the presence of several of my medical friends,) I separated the lips of the wound with a probe, introduced my director and let off another pint (by measure) of water. The child appeared to suffer as little pain as before, from the operation; the bones of the head were more firmly attached to each other, and I was enabled to apply the necessary bandages with much less difficulty.

My patient, on the morning of the sixth of March, was very languid and to appearance much worse. I discovered, that there had been a very considerable discharge of water from the incision during the night, and that the child was much exhausted in consequence of it. The lips of the wound had not united, by the first intention, as after the first tapping, and the water had continued to ooze through the orifice, from the time my director had been withdrawn. I immediately applied a small dossil of lint to the part, and when I visited the child in the evening, I was happy to find her somewhat recovered.

On the morning of the seventh of March, I found her as free from pain, as before the operation, and much revived, having sucked heartily, and having had a good night's rest. I discovered however, that the discharge of water from the orifice having ceased, its accumulation had evidently increased, and the fluctuation had become more distinct.

It became necessary on the morning of the eighth of March, to repeat the operation of tapping the head, for the

third time, which was done in the presence of several medical gentlemen, in the manner last mentioned, and another pint (by measure) of water was drawn off. The child did not appear to suffer more pain than from the previous operations.

When I visited the child on the morning of the ninth of March, she was as well as usual; but, on examining her head, I discovered that the water was again accumulating.

On the morning of the tenth of March, the child being still as well as usual, and finding the water had increased considerably in quantity, I resolved again to let it off, before the incision had healed, in order to supersede the necessity of making a new orifice. This I accordingly did, in the presence of several of my medical friends, and let off three gills (by measure) of water, making three pints and three gills, which were drawn off in the four several operations of tapping the head, from the third to the tenth of March; all of which were borne by my patient with very little apparent suffering, and evidently with the happiest result, for the child had become more fleshy; and if the water did not cease to collect, its accumulation was now slow.

From the tenth to the twentieth of March, my patient continued to improve in health. The bones of the cranium had approximated considerably, and were uniting at their sutures. The deposition of water was inconsiderable and its accumulation slow. Her eyes were very little distorted. She was able to move her head; knew her mother; sucked heartily; passed a considerable quantity of water daily by urine; and in fact every favourable symptom presenting itself to our view, we had every thing to hope.

On the morning of the twenty-first of March, the child's head measured only nineteen inches over the frontal and occipital bones, and an inch more from under the chin and over the ossa parietalia; after an interval therefore of ten days from the last operation, it was very little larger than when I first saw the child, which was on the thirteenth of December, 1817, and her health had now much improved.

On the twenty-second of March, I found the child very

sick; she had been distressed through the night (from taking improper food) with cholera morbus; was much reduced and very languid, being scarcely able to suck without her chin being supported by her nurse; she had likewise had two or three convulsions through the night. I ordered her an absorbent anodyne mixture.

On the twenty-third of March, my patient was still very ill, having had nineteen convulsions within the last twenty-four hours.

On the twenty-fourth of March she was still very ill, but had had only two convulsions during the preceding twenty-four hours.

On the twenty-fifth of March, she was still ill; she had had only two convulsions during the preceding twenty-four hours.

On the twenty-sixth of March, I found my patient somewhat better; she had been free from convulsions, and continued so until the night of the twenty-ninth, when she had another. We finally succeeded in relieving her bowel complaint, and with it her convulsions ceased. The plan of treatment which I pursued in the cure of this unwelcome and unexpected innovation of another and distinct disease, it is unnecessary that I should mention here. I am sure it would not be new to you.

No sooner was my patient relieved from her attack of cholera morbus, than I perceived, that the water was again accumulating within the cranium. She notwithstanding continued to acquire strength, and improved in her general health, until the 14th of April, 1818, when the quantity of water effused, indicated the necessity of another operation. I determined to repeat it for the fifth time (her head measuring 18 and 20 inches in the two several directions hitherto mentioned) but, here, a difficulty arose; little or no fluctuation of water could be discovered at the part where I had previously operated so often with safety and success. The parietal and temporal bones had united at the squamose suture, and it became necessary to make the incision in another place. The fluctuation being most distinct in the

course of the coronal suture, I made the incision on the right side of the child's head, at an equal distance between the frontal and parietal bones (these bones being separated at their sutures) and at about an equal distance from the sagittal and squamose sutures, and finished the operation as before. I succeeded in drawing off another pint (by measure) of water, in the presence of Drs. Porcher and Waring and several of my pupils. Only a few drops of blood escaped from the incision, and nothing unfavourable occurred during the operation; but, when it was over, I perceived, that the head had been more completely evacuated of water than from any previous tapping; that the collapse of the integuments was greater, and that the cavity at the anterior fontanelle was large enough to receive a hen's egg; the child too became pale, her pulse was languid, she was likewise threatened with vomiting, and shewed evident symptoms of a disposition to faint. She however revived in a few moments after the dressings were applied, and sucked tolerably well. I have since thought that we drew off more water at this tapping than the child bore with convenience, or than was proper. The medical gentlemen present were of opinion, that the child's eyes were much less distorted after, than previous to the operation.

In the evening, on visiting my patient, I was informed by her mother, that she very soon recovered from the effects of the operation; that she was then very easy, and had slept several hours.

On the fifteenth of April, my patient was much better. She had passed a more tranquil night than she had done for a fortnight, and without taking an anodyne; she sucked well, and squinted less than I had ever before observed. Some water, however, was again discovered to be effused within the cranium, and the head had in some degree recovered its fulness. No oozing had taken place from the incision of the day before.

My patient was as well as usual on the sixteenth of April. Her mother then informed me she had observed, that when her child had been recently tapped and there was little wa-

ter present in her head, she was enabled to direct her eyes to any object; but, that when the collection of water was considerable, the reverse was the case; that then strabismus and an involuntary motion of the eyes was always present. Some water having now collected, and the last incision having not yet healed, I resolved to introduce my director for the purpose of letting it off the sixth time. I did so, and discharged a gill (by measure) which I believe to have been all which had collected, as the subsequent collapse was considerable. She bore the operation extremely well, and appeared to suffer very little pain from it.

From the sixteenth of April to the eleventh of May, my patient continued to improve in health, and nothing occurred worthy of notice.

On the twelfth of May, business of an imperious nature obliging me to leave the city, I committed the care of my patient to Dr. H. Waring, who informed me on my return, that the child had several convulsions on the nineteenth and twentieth, and that on the twenty-first the accumulation of water made it necessary for him to repeat the operation, (being the seventh time) which he accordingly performed in the presence of Dr. Whitridge, when three gills of water were drawn off. He likewise observed to me that the child had two convulsions a short time previous to his operating, but that they ceased to return after the head had been tapped.

The child soon recovered from the effects of the operation, and when I returned to the city on the twenty-third of May, found my patient as well as usual. The water however, I shortly after perceived to be still accumulating, and on the thirteenth of June it became necessary to tap her head the eighth time. I performed the operation as before, in the presence of several of my pupils, and a pint of water (by measure) was again drawn off. Her head measured in circumference, previous to the operation, twenty inches over the frontal and occipital bones, and twenty-four inches from under the chin and over the ossa parietalia. The child suffered as little from this, as from any of the

previous operations, and I at length began to flatter myself that my endeavours to relieve my little patient would ultimately prove successful.

In this, however, I was disappointed. A few days only had elapsed when I discovered my patient to be much worse. I found her gums much swollen and occasioning her considerable distress; she likewise had thrush, and evident symptoms of whooping cough.

On the morning of the seventeenth of June, she had very high fever, and I was informed by her mother that she had had no sleep during the night, having been excessively distressed with paryoxsms of convulsive coughing. Her fever continued without intermission, attended with cough and occasional convulsions, until the morning of the twenty-first of June, when all my efforts to relieve her proving ineffectual, she died.

It is perhaps worthy of notice, that, so soon as the febrile action commenced in the system, the accumulation of water in the head became much more rapid, than was discovered at any previous period of the disease. It was likewise uniformly observed, that after each tapping the kidneys did their duty more freely for several days, and there was much more water discharged by urine, than at any other time, even when they were stimulated by diuretics.

The head, after death, measured twenty inches in circumference over the frontal and occipital bones, and twenty-four inches from under the chin and over the ossa parietalia.

On tapping the head, three pints of water (by measure) were taken away, (in the presence of Dr. H. Waring, my brother Dr. H. C. Glover, and several others,) making in the whole nine pints and three quarters which were drawn off in this case in the short period of little more than three months, from a child who was only seven months old.

The head being opened, I found that the opinion which I had given of the case was correct. The water had been contained between the dura and pia-mater. The dura-mater was much thickened, but shewed no marks of acute inflammation.

The brain was nearly all absorbed, but what remained of it, rested with its pia-mater on the base of the cranium. It had lost much of its consistence; indeed I found it impossible to distinguish the several parts of which it was composed, and on appealing to the gentlemen present, they, as well as myself, were of opinion, that it was not larger than a hen's egg; and yet, astonishing to relate, this child retained her senses to the last, and appeared to possess many of the faculties of the mind.

I cannot conclude the statement of this case, without recommending the operation of tapping the head in this species of hydrocephalus, in the strongest terms. I believe it to be the surest, safest, and most speedy method of relieving, if not of curing this very formidable disease, and I have now to regret that I did not have recourse to it earlier in the case which I have just related.

It will be recollected, that from the thirteenth of December 1817, the day on which I first visited this patient, to the third of March 1818, the day of the first operation, the disease had increased with great rapidity, under various plans of treatment. No sooner, however, did we have recourse to the operation of tapping the head than the disease, to say the least of it, was arrested. Had we therefore commenced with it earlier, it is evident the chances of success would have been greater.

ART. XI. *A Case of Gangrenous Suppuration.* Read before the Academy of Medicine of Philadelphia, and communicated by JOHN BARNES, M. D. one of the Vice Presidents of the Philadelphia Medical Society, &c.

ON the 10th of July, 1820, I was requested to visit Mr. R. aged about 40 years. He informed me, that for several days past, he had felt considerable uneasiness within his rectum, which was increased by costiveness, and that at

the moment of passing his fæces, the pain was greatly augmented.

There was no external appearance of disease, and as the symptoms were precisely the same, as those which result from an enlargement of the hemorrhoidal vessels internally, I believed the complaint to arise from this cause, and accordingly prescribed rest in a recumbent position, laxatives, and a low diet. By these means his symptoms, for a few days, were somewhat mitigated—though not essentially relieved.

From tenderness of the parts, he was unwilling to submit to an examination per anum—and as there was, as yet, no appearance of disease externally, I was obliged to be content with such information as the patient gave,—and in addition to the remedies already prescribed, directed a large emollient anodyne poultice to be applied over the anus, and its vicinity.

No material variation took place in the case until about the sixth day of my attendance, when there was observable on the left side of the anus, a preternatural fulness, which did not vary in colour from the surrounding parts, and which was not perceptibly harder than natural. The same plan of treatment was continued till about 8 o'clock, A. M. of the 23d inst. when upon examination, that part of the buttock, situated on the left side of the anus and immediately in connection with it, where a fulness had before been observed, was found considerably enlarged and hard—and an erysipelatous inflammation had commenced at the verge of the anus, which extended about two inches in diameter. Considering the case now, as an attack of erysipelas, I ordered the poultice to be discontinued, and its place supplied by a lotion, consisting of eight ounces of lead water, in which had been dissolved a drachm of opium, and cloths wet with this were directed to be kept constantly applied.

I called about six o'clock, P. M. of the same day, and found my patient in the greatest agony, with a sunken countenance, heavy eye, and feeble pulse.

The erysipelatous inflammation had now extended over nearly all the space covering the glutei muscles of the left side, and the part occupied by it was hard, swollen, and of a purplish red colour, resembling exactly, in appearance, a large carbuncle approaching its sloughing state. Near the middle of the diseased part, there was a vesicle elevated about three lines above the surface and extending about eight lines in diameter, filled with a fluid perfectly black—and the whole surface of the inflamed part presented so dusky an aspect, that I really considered the recovery of my patient as almost hopeless. From the great prostration of strength which had already taken place, it was clearly manifest, that should the parts embraced by inflammation mortify—and appearances seemed strongly to indicate such a result—death must inevitably ensue: the only chance, therefore, which seemed to remain for the prevention of such an event, was instantly to arrest, if possible, the progress towards mortification, and to sustain the sinking powers of the system. Tonic medicine and generous diet, were accordingly prescribed, and a blister applied, so large, as to cover all the diseased part, and extend some distance over the sound skin.

I visited my patient again about ten o'clock, P. M. near four hours after the application of the blister, and found him in a sound sleep, with a pulse rather slower and fuller than when I last felt it.

Encouraged by these favourable indications, I now entertained some hopes of his recovering, which were fully confirmed on my visiting him early on the morning of the 24th, for his pulse was now slower, fuller and more regular, his eyes somewhat brighter, and his countenance no longer exhibited that ghastly, sunken aspect which was so appalling on the preceding evening. He also informed me, that the horrible burning sensation from which he had suffered so much during the preceding day, ceased soon after the application of the blister—that he had slept more soundly than he had done for some time, and that he now felt as if he were in another world.

The blister I found had drawn much more than could reasonably have been calculated, having even raised over the parts most inflamed, where, we should have supposed, that the morbid action was too great to be superseded by that of the catharides.

Excepting the anus, where there was a pouch-like appearance, which was open, and discharging a large quantity of thin, dark and very fetid pus, the diseased parts had greatly diminished in size. I directed the carbonized effervescent poultice to be applied, and the strength to be supported by nourishing diet and malt liquors.

Under this plan of treatment, the parts continued gradually to resume their natural healthy condition, which, however, was not fully accomplished until the termination of the third week from the application of the blister.

During the greater portion of this time, the fetor of the discharge was so intolerable, that we were obliged to employ almost constantly, acid fumigations.

Mr. Samuel Cooper in his Surgical Dictionary, under the article Anus, has the following observations.

“ Sometimes there is formed in this part (vicinity of the anus) what the French call *une suppuration gangreneuse* ; in which the cellular and adipose membrane is affected in the same manner, as in the disease called a carbuncle.”

“ In this case the skin is of a dusky red or purple kind of colour; and although harder than when in a natural state, yet it has by no means, that degree of tension or resistance, which it has either in the phlegmon or erysipelas.”

“ The patient has generally, at first a hard, full, jarring pulse, with great thirst, and very fatiguing restlessness. If the progress of the disease be not stopped or the patient relieved by medicine, the pulse soon changes into an unequal, low, fluttering one ; and the strength and spirits sink in such a manner as to imply great and immediately impending mischief.”

This description of the gangrenous suppuration of French writers corresponds so nearly with the leading features of Mr. R.'s case, that I am led to consider them as similar.

In examining the various remedies which have been recommended for this particular form of disease by *European* authorities, we find such only as mitigate the sufferings of the patient, and support the tone of the system, without having any direct tendency to change the morbid action of the part. I feel perfectly confident that had such remedies only, been employed in the case of Mr. R., he would have sunk beneath the ravages of his disease. The preservation of life in this case, I ascribe entirely to the blister. It not only alleviated that state of suffering in which anodyne applications proved so unavailing, but instantly arrested the morbid action, which menaced such fatal consequences.

I have been induced to lay this case before the medical public, that the younger members of the profession may be apprised of the rapid manner in which this form of disease tends to undermine the vital powers, after its erysipelatous state has commenced—and also to furnish another proof to the many, which have already been advanced, of the great importance of that *original precept* laid down by Professor Physick in his surgical lectures—that we should vesicate in violent forms of erysipelas, in anthrax and in gangrene and mortification, proceeding from *morbid action*, under either of which shapes of disease, this case might, with no great degree of impropriety, be arranged.

ART. XII. *A Case of Carbuncle, with some remarks on the use of Caustic, in that disease.* By PHILIP S. PHYSICK, M. D.

ON the twenty-fourth October, 1820, I was desired to visit Robert Wharton, Esq. the present Mayor of Philadelphia, to examine a very painful bile on the back of his shoulder. It had commenced five days previous to my visit, was hard, about one inch in diameter, and in the middle of a purple colour. The disease was evidently a small carbuncle, and Mr. Wharton was advised to remain at home, to

avoid using the arm of the affected side, and to cover the swelling with a soft linseed poultice.

Next morning he informed me, that being occupied with some very urgent business, he had not been able to confine himself, and that though taking every care in his power to avoid injuries to the part, the pressure of his clothing during the preceding day, occasioned him great pain. On examination, I found the swelling very considerably increased, so that its diameter was now more than four inches—he felt the necessity of remaining in his chamber, had a poultice applied, and took an anodyne draught. Having no fever, he was allowed his accustomed food and drink.

The following morning, which was the 7th from the commencement of the disease, I was informed, that though the pain continued, he had some sleep during the night, and had taken his breakfast as usual with a good appetite.

On the eighth day, I found he had suffered considerable pain all the preceding day and night, notwithstanding an anodyne had been taken both in the morning and evening. The decoction of Peruvian bark was now ordered. Next morning, Mr. Wharton complained that the pain had increased to such a degree of severity, as to deprive him entirely of sleep. The pain indeed, was of so depressing a nature, that he felt himself very much weakened by it, and he had some nausea. An appearance of two or three pimples was now observed on the middle of the swelling, which more carefully inspected, were found to be small orifices formed through the skin. During the whole of this day, the pain continued extremely severe and distressing. I observed in the morning of the tenth day, that the orifices were enlarged in size and in number, and the pain increasing, occasioned still more nausea, and an entire loss of appetite.

At this period, I suggested the application of the caustic vegetable alkali upon the middle of the tumour, for the purpose of destroying all that part of the skin perforated by the orifices just mentioned. This was immediately submitted to, and an eschar formed of about two inches in diame-

ter. The pain from the caustic ceased in a quarter of an hour, and from that time Mr. Wharton suffered no pain whatever, from the disease. The nausea, and uneasy feelings of his stomach, subsided entirely—his appetite became good—he slept well—the eschar separated, and the mortified parts were gradually discharged through the opening made by the caustic. The cavity, which was a large one, granulating finely, filled up, and at the end of four weeks from the beginning of the disease, the external sore was completely cicatrised.

My object in the above detail of Mr. Wharton's case, is to add my testimony to that of some other surgeons in favour of the use of the common caustic vegetable alkali in the treatment of carbuncle. I am well aware, that a difference of opinion has existed among practitioners, respecting the application of caustic, some recommending it, and others objecting to it as pernicious. This difference appears to me to have arisen from the remedy having been applied at different periods of the complaint indiscriminately.

With a view of communicating the method of using the caustic which I wish to propose, the progress of carbuncle may be divided into three stages. The first stage is that in which the disease is forming, and in which the peculiar inflammation exists in the cellular texture under the skin, that ends in its death or mortification.—This is attended by a burning pain and sometimes by fever—Carbuncle in this stage seldom proves fatal, unless it be very large, or when situated on the head, or where the constitution has been previously impaired.

The second stage is that, in which pimples appear with orifices through the skin, which gradually increasing, join, and eventually form one or more large openings, through which the mortified parts, and acrid fluids, pass out. These effects are produced by the ulcerative process, which during the whole time of its continuance, is attended with the most severe and distressing pain that is experienced in the course of the disease.—The patient's constitution also, suffers so materially, owing to loss of appetite, want of sleep and fe-

ver—that death is the consequence, in severe cases—and in others the recovery is difficult and tedious.

The third stage is that, in which an ulcer remains, attended, however, with no peculiarity from its cause.

In the first stage, all irritating treatment appears to be very injurious, by increasing the peculiar inflammation then existing, and thereby extending it. Hence the bad effects of incisions, scarification, and stimulating applications in the beginning of the complaint, and such too, is the reason why caustic has been sometimes found injurious. From the great power of blisters in checking mortification, when proceeding from some kinds of inflammation, I once entertained high expectations of their utility in the treatment of anthrax. But though I have found them serviceable in abating the burning pain attending the inflammation, they have not shown any power in counteracting its progress to mortification.

In the second stage, the inflammation having ended in the death of the cellular texture in which it was situated, a process begins for making an opening through the skin to allow the dead parts, and acrid fluids, to pass out. The commencement of this process is pointed out by the appearance of pimples and small orifices as above described, and it is at this period, that the application of caustic vegetable alkali upon the skin so perforated, and on that covering the middle of the tumour, in quantity sufficient to destroy it completely, proves highly beneficial.—In all the cases in which I have used the caustic in this manner, the suffering of the patient ceased, as in Mr. Wharton's case, as soon as the pain from the caustic subsided.—It operates by destroying in a few minutes that portion of the skin covering the mortified parts, which, if left to be removed by ulceration, would require several days for its completion, occasioning the chief part of the pain and danger attendant on and consequent to the disease.

REVIEW.

ART. XIII. *The History of the Practice of Vaccination*, by JAMES MOORE, Director of the National Vaccine Establishment, Surgeon of the Second Regiment of Life Guards, and Member of the Royal College of Surgeons, in London.

“Cum ea, quæ quasi involuta fuerunt, aperta sunt, tum inventa dicuntur.”
Lucullus, Cicer.

BY those who are studious only of novelty, it may possibly be asked, why we have, at this time, chosen to review a subject, which has become trite by the frequent and copious discussions it has received. To all such enquiries we would reply, that there is none more curious in itself, or has stronger claims to attention, than vaccination, in whatever light it may be viewed. Whether we regard the mode in which the discovery of the disease was made, or the history of its progress, or the peculiarity of its pathology, or the immense benefits it has conferred on mankind, it is replete with interest and importance.

To these just titles to our notice, a new one of a widely different nature, has lately been added. No longer is it to be concealed, that there is now every where, in this country as well as Europe, more or less distrust of the powers of vaccination in the prevention of small pox. What has occasioned the loss of confidence in this process, once nearly universal, it becomes us, as a very momentous concern, carefully to enquire, and accurately to determine.

Entertaining the conviction, indeed, that a candid examination of the subject in all its leading relations, cannot fail to be useful, and especially to a certain section of our readers, we shall enter as freely into it as the limits, to which we are unavoidably restrained, will allow. As the work before us treats with ability, many of the points involved in the proposed discussion, the execution of our design will be

much facilitated by a summary of its contents, which we shall accordingly, in the first instance, attempt.

Could we confide in accounts merely traditionary, or resting on authority quite as questionable, we might conclude that as a preventive of small pox, vaccination had been known in some places for a century, and even a longer term, and such is stated to be particularly the case in parts of England, Wales, Ireland, and the Oriental countries.

Yet, whatever credit may be due to these vague reports, it is manifest that from an acquaintance with the disease, thus said to have existed, no essential advantage was derived.

Notwithstanding the early difficulties and opposition he had to encounter, and the many and persevering attempts, open and disguised, which were made to despoil him of his well earned applause—to Dr. Edward Jenner, of England, is due, the honor of having introduced and established the discovery of Cow Pox. Convinced of the validity of his claims, to remunerate, in some measure, a man who had shed a lustre on the country and age in which he lived, and so essentially benefited the human race, the British Parliament, after the most deliberate investigation, voted him the sum of thirty thousand pounds.

It appears, that about the year 1768, while Jenner was a student of medicine, residing in Gloucestershire, he heard of the prevalence of a complaint on the udders of cows, of a pustular nature, which infecting the hands of those who milked them, produced a correspondent affection, proving a preventive of small pox. These milkers, often applying to his preceptor for remedies to cure a very curious species of fester on their fingers to which they were liable, afforded him an opportunity of examining the pustules, and he was assured that they were derived from similar ones on the teats of cows. The fact he moreover ascertained to be well established and of notoriety, that the peasants of that tract of country, even under repeated inoculation, in many instances, resisted the infection of small-pox.

Deeply impressed with the value of this intelligence, he repaired to London to prosecute his studies, under the aus-

pices and roof of the late Mr. Hunter. During the period of their connexion, they held repeated conversations respecting the cow-pox and its reputed effects. But the facts alleged not having undergone a rigid scrutiny, and the authorities on which they were reported being still equivocal, engrossed, as he was, with other pursuits, Mr. Hunter did not deem the subject worthy of further inquiry.

Completing his studies in London, Mr. Jenner established himself as a surgeon in his native town of Berkeley, indulging his leisure in philosophical pursuits, and more especially in a steady and unwearied research into the cow-pox. This may be considered as the real commencement of that train of investigation which eventuated in the discovery that has thrown around his name imperishable renown, and enrolled it with those

"Inventas aut qui vitam excoluere per artis,
Unique sui memores alios fecere merendo."

After an elaborate and long continued inquiry, in the course of which he evinced great sagacity, overcoming difficulties which we cannot recount, and performing experiments we must not consume time in enumerating, he conclusively proved, that the cow-pox may be communicated by regular inoculation, passing from one individual to another, without deterioration or change—that it is a preventive of small-pox, being itself a much milder disease, and not communicable, except by inoculation—and that, it is not productive of any secondary complaints.

Tracing still further the chain of causation, he also ascertained, that a remoter source of cow-pox is in an affection of the horse's heel, denominated *the grease*, which is imparted to the cow by milkers, previously handling the infected foot of the horse and neglecting the proper degree of cleanliness.* Experiments made by Mr. Coleman, the well known veterinary surgeon, it is true, were in contradiction to these views. But they have been since abundantly confirmed by the concurrent reports of various authorities, and seem now to be received as no longer disputable.

* It is a common practice in many parts of England for men to perform the double office of attending to horses as grooms and of milking cows.

Being satisfied of the accuracy of all these results, Dr. Jenner resolved to lay them before the public, through the medium of the Transactions of the Royal Society. But so little confidence did that learned body place in his statements, that his paper was returned with a friendly admonition, to withhold it from the press, lest it should injure his reputation. The advice, happily, was not followed, and in 1798, he promulgated in a small printed pamphlet, in a style the most modest, and unpretending, his great discovery.

As a preventive of small-pox, vaccination from this period came gradually to be diffused throughout the civilized world, meeting in different countries, various degrees of resistance, according to the extent of prejudice, or the inefficiency of exertion in its behalf.

In the United States the discovery was early adopted, and with little or no division of sentiment as to its utility. Commencing in this city and Boston, from these points it was speedily diffused in every direction over our wide spread territories. But this is denied by our author, who professing to give a faithful history of the progress of the discovery, has in too many instances, we regret to say, indulged in a strain of invective and intemperate feeling, neither called for by the occasion, nor consistent with his own enviable name and distinguished reputation.*

"The freedom," says he, "that reigns in the United States of North America, is incompatible with unanimity; consequently, the vaccine had to struggle there with a long and violent opposition, which was not much allayed by the exertions of the president Mr. Jefferson, who patronized the new practice: yet by degrees it spread, and was introduced even among the Indian tribes. It was in the year 1799, that this important benefit was conveyed to the United States from Great Britain. Indeed, except the produce of the soil, what that is valuable, has that nation not received from this? Certainly their arts, literature, laws, religion, the model

* Mr. Moore, is the son of the celebrated author of *Zeleuco*, and the brother of the late Sir John Moore, and of admiral Graham Moore, and holds an eminent rank in his profession.

of their political establishments, and even their love of liberty. Yet, when Britain was hard pressed by Napoleon, who by furious and successful enterprise had forced the European nations into a league against her, the United States submitted to the threats and depredations of the tyrant, and joined their forces to enslave their parent country, the restorer and last shelter of liberty in Europe. American diplomatists have exerted much political subtlety to apologise for combining in this miscreated scheme, which would have been scorned and opposed by the virtuous Washington.

“But let England forget this, and rejoice in being able to add the vaccine to the other benefits conferred upon the Americans. And may our physicians continue to instruct them to cure and prevent the diseases of their country; may our poets soften and delight them; and, above all, may our philosophers improve their dispositions; and perhaps in a future age, their animosity will cease, and there will spring up in that country some filial gratitude.”

It is impossible to read the above extract, without feeling a mixture of indignation and contempt at the wanton deviation of the writer from his immediate subject, to indulge in such miserable invective. The passage is altogether a curiosity, and seems a pretty good specimen of the temper and impotent malice so often entertained and practised by Europeans against us. Mr. Moore is wholly without excuse. In tracing the progress of vaccination, he must have met with abundant evidence that in relation to us the fact was precisely the reverse of his statement. No where, we repeat, was vaccination more eagerly adopted, or has encountered less opposition than in the United States. By the common consent of regular practitioners, to variolate has long been made a matter of professional reproach, and in consequence, small-pox may be considered as nearly extinct within the limits of our country. Consulting the bills of mortality of five of our largest cities for the last year, namely Boston, New York, Philadelphia, Baltimore, and Charleston, we do not perceive the record of a single death

from that disease, and probably there was no one case of it in this immense population.

Could we descend to such pitiful warfare, it would be easy to prove from her own writers, including Mr. Moore, that Great Britain herself merits these imputations, in a pre-eminent degree. To this end, it is not at all necessary to cite any part of the phrenetic ravings of Mosely, Squirrel, Birch, Rowley, Lipscomb, or the other infuriated fanatics, whose names swell out the long catalogue of antivaccinists. The work of Mr. Moore alone, will supply all that the most vindictive spirit of retaliation could desire. Examining his narrative, we shall find page after page, filled with lamentations at the cruel injustice done to Jenner and his fine discovery,—of the stupid insensibility of his countrymen to their value, while recognised by all the rest of the civilized world, illustrating the well known maxim, that a prophet has no honour at home, with several other expressions of similar tone and purport. “It becomes Englishmen in particular,” says Sir Gilbert Blane, in his late tract on vaccination, “to foster it, not only as the native offspring of his country, of which he has reason to be proud, but to redeem the character of the nation from the reproach of having of all others, whether savage or civilized, done the least justice to this noble discovery. There is no country which has prized it less, or availed itself of it so little. To those nations who may feel an envy of the glory attached to our country by this discovery, it must be no small consolation to perceive that a large portion of the English nation has hitherto been so besotted as not to know how to appreciate nor avail itself of it, and that it has encountered more opposition among ourselves, than in all the world besides.”

Never, perhaps, did the profession suffer so much in reputation, or the human mind, indeed, more abasement, than in the contentions of the English Polemics on this subject. Commenting on them, the Edinburgh Reviewer employs the following strong language. “Charges,” says he, “of murder and falsehood were interchanged among the disputants with-

out the smallest ceremony,—the medical journals foamed with the violence of their contention,—it raged in hospitals and sick chambers, and polluted, with its malignity, the sanctity of the pulpit, and the harmony of convivial philanthropy.

“In the whole of our censorial labours, we never had occasion to contemplate a scene so disgusting and humiliating, as is presented by the greater part of this controversy; nor do we believe that the virulence of political animosity, or personal rivalry or revenge, ever gave rise, among the basest and most prostituted scribblers, to so much coarseness, illiberality, violence and absurdity, as is here exhibited by gentlemen of sense and education, discussing a point of professional science with a view to the good of mankind. At one time, indeed, we were so overpowered and confounded by the rude clamour and vehement contradiction of the combatants, that we were tempted to abandon the task we had undertaken, and leave it to some more athletic critic to collect the few facts and the little reasoning which could be discerned in this tempest of the medical world.” To all this, what will Mr. Moore reply?

By some it was supposed, that the vaccine and variolous diseases were originally the same, the latter having received its peculiar modification by passing through the human constitution, in a long succession of generations. But independently of the essential difference in the nature of the two diseases, as they apparently now exist, there are some facts which give nearly a positive refutation to the notion of their primitive identity.

It is ascertained, in the first place, that variola is peculiar to the human species,—all attempts to infect the brute creation with it having failed. Experiments, too, prove that the two diseases are not susceptible of intermixture, each preserving its distinctive peculiarities under all circumstances. Thus it is found, that if inoculation be performed with the two fluids blended together, sometimes the vaccine, and at other times the variolous disease will be excited, without the slightest change of character from the process. Equally is it shown, when these fluids are inserted separately, though so conti-

guously, that a common pustule is produced, either the one or the other disease will be raised, according to the side of the pustule from which the virus may be taken. Even more conclusive, is the fact, that the two fluids being introduced at the same time, each proves effective, evolving the respective diseases. Yet, to a certain extent, their actions are reciprocally restrained, the vaccine pustule is smaller, and proceeds slower to maturity, while the variolous eruption suffers pretty much in the same way. Delay, however, variolation till the ninth day, when the constitutional affection takes place from vaccination, susceptibility to the former is completely destroyed, and there is no general effect.

We are aware, that on this point in the early history of vaccination, an opinion somewhat different prevailed, and that it was inculcated, that the genuine vaccine pustule might be induced in systems which had been previously subjected, either to small pox, or to cow pox. The repetition of the process as regards the vaccine disease, was indeed, as well as we recollect, recommended by Jenner himself, as the best means of transmitting the virus to distant countries. But the fallacy of the opinion has long since been exposed, and the practice of course, entirely abandoned. As demonstrated by an infinity of trials, the vaccine or variolous matter, has, on a system circumstanced as above stated, only the power of creating local inflammation, or at most, an abscess like an ordinary poison or irritant.

In the practice of vaccination, certain rules are recommended and should be carefully observed. The first of these regards the time for the performance of the operation. Except under very peculiar circumstances, it is not to be done sooner than the third week after birth. The uncertainty of organization being complete, and the extreme delicacy and irritability of the new born babe, are the grounds of this advice. None of the trivial derangements of health incident to the month, constitute objections to the operation. But where crusta lactea, or herpetic eruptions, or, indeed, any cutaneous disease exists, it is for the most part inadmissible. It is exceedingly difficult, in this state of the skin,

to get the virus to act, and when it does, it is apt, as we shall presently show, to generate a mixed disease, destitute of the property of protection against small pox.*

Concerning the selection of the virus much has been said, and it is doubtless of great consequence. European writers, including Jenner, seem generally to prefer the pellucid fluid directly from the pustule, and urge its being taken on or before the ninth day, or, in other words, prior to its becoming opaque or purulent, or the areola being formed. That it is less active after this period, seems pretty well established, though we have ample evidence that it is still efficient, provided the vesicle has not been opened, or its peculiar actions disturbed in any mode. Exposed, however, as it is to these contingencies, perhaps it would be right to abandon the use of the transparent virus altogether, and substitute the scab. To the latter practice we know of no objection, and it is supported by several considerations.

First.—It allows the disease to run its course, free from the danger of changing its specific character, by any artificial interference.

Second.—The virus embodied in the scab may, by the ordinary precautions, be much longer preserved, even for a year or upwards without vitiation or diminution of power.

Third.—It affords a larger supply of matter for an extensive propagation of the disease.

Fourth.—The difficulty of infection is lessened by the ample portion of virus which it furnishes.

Be it remembered, however, that there is a choice in the scab. Those which are *hard and compact*, of a *dark mahogany* colour, and with a *regular well defined* margin, should be uniformly selected. The *pale grayish* scab, *scaly* or *lamellated* in its structure, with *ragged edges*, is always suspicious, very liable to fail, or if it infects, produces an illegitimate disease.

* We are told by Jenner, that even the sulphurous impregnation of the skin, which takes place in the case of itch, prevents vaccine infection. To this purport, he relates the fact of his inability to communicate the disease to a body of soldiers in this condition, who, after using the warm bath, very readily received it.

In employing the scab, all the loose parts which lie on the inner surface, and attach to the circumference, are to be scraped off, and a small portion of the real solid scab is to be pulverized and moistened to the state of a thick or ropy fluid. As in the case of the pellucid virus, this may be inserted through a small puncture or scratch, or what succeeds better, to lay it on the skin, and work it in with the point of a lancet, taking care not to cut so deep as to occasion bleeding, which washes away the matter, and defeats the operation.

The history of the progress of the disease may be told in a few words. It almost invariably happens that the incision heals so as scarcely to leave a vestige—any appearance to the contrary usually denoting common inflammation instead of the specific action of the virus. The infection succeeding, there may be seen, about the close of the third day, a small red speck, somewhat elevated, which, on pressure, imparts to the finger the sensation of its enclosing a hard substance. This minute tumour gradually enlarges, and about the sixth day a small vesicle is formed, having a round margin, a flat surface, with a slight indentation in the centre, is of a pink colour, which changes to a deeper red, with a mixture of blue, and is darkest in the middle. On the seventh, eighth or ninth day, for the time is not very precise, there is thrown around the base of the pustule an efflorescence like a ring, which spreads rapidly, and in one or two days becomes a florid areola of an inch or more in diameter. The vesicle is now at its height, and the efflorescence throughout its whole extent is tumified, in which state it continues for several days, and then subsides and fades away. In the declination of the vesicle, the centre darkens first, and the whole by degrees is converted into a hard smooth crust of a mahogany complexion. The crust drops off spontaneously in the course of the third week, leaving a superficial cicatrix slightly indented by the cells of the vesicle.

This is the development of the local affection. About the expiration of the seventh day, in adults, or children some

what advanced, in whom there is usually most disease, the ordinary symptoms of fever are manifested, and sometimes even to a considerable height. But these are generally slight and evanescent,—the only serious complaint being soreness and tumefaction under the axilla, which even in infants exist—and hence we should be careful that they be not raised by the arms.

More, than any other point, is it important to understand the diagnosis between the genuine and spurious disease, and no great attention, as to ordinary instances, is necessary to acquire it. The peculiarities of the real pustule, we have already and so recently described in all its stages, that we shall say nothing further regarding it. In the spurious disease, “there is a premature itching, inflammation, and suppuration,—the progress of the vesicle is too rapid—its texture is soft—the edge not well defined—the centre elevated—the matter discharged is purulent—it is also pointed like a common abscess. Instead of a regular areola, a premature efflorescence, of a dusky purple colour, takes place, and the crust is of a light brown or amber colour.

“The irregular vesicle is more liable to be broken than the regular, both from its form and texture—and also from its being usually so irritable as to provoke scratching. When broken, this vesicle is apt to ulcerate.

“A vesicle apparently regular at first, sometimes does not increase to the proper size, but dies away prematurely: such a vesicle usually leaves no cicatrix, or one which is hardly perceptible.”*

What is very curious, the constitutional affections are more violent, though no protection is afforded against small pox, or subsequent vaccination. Little importance can be placed on the appearance of the cicatrix or scar. This is sometimes wide and deep, from the ravages of previous ulceration, more so, indeed, than in the legitimate cases. The genuine pustule commonly leaves a slight, superficial indented impression—whereas, the spurious is followed by a white, shining, strong marked scar, resembling that from a suppu-

* Vide the Instructions of the National Vaccine Establishment of London.

rating wound. Cicatrixes of this sort, have been usually remarked in those cases where small pox has succeeded to vaccination.

Touching the treatment of cow-pox, we shall say very little. Children are hardly ever so sick as to demand medicine, or even any material change of diet. Evacuations of the bowels by the mildest laxatives, and occasionally small doses of the dulcified spirits of nitre, we have found sufficient in the worst cases. But should an attack assume a more violent shape, which is affirmed now and then to have occurred in grown people, the management is to be conducted on ordinary principles, and by the customary means suited to the particular indications.

The local affections, however, more frequently call for attention. To allay excess of inflammation, cold water, or water and vinegar, or a weak solution of acetate of lead, may be applied, and to arrest or heal the ulcer, the common dressings are to be used. In some of these instances, we have found the blue mercurial ointment, or the citron ointment, or simple cerate with calomel, singularly serviceable.

No medicine is required to prepare the system for the reception of the disease, or its subsequent purification, as is vulgarly believed, from the taint which it imbibes. We shall conclude this part of our subject with stating, that after using all the precautions already directed, the patient is to be visited on the appearance of the vesicle, at its maturity, and at its declination, watching to determine whether it goes through these several stages with regularity.

The question again recurs, in consequence of recent reports to the prejudice of vaccination, whether, on the whole, it is still entitled to confidence? That cases of its failure of protection against small-pox have within the last two or three years, vastly multiplied, seems to be confessed. But, perhaps, in a fair estimate, these are not to be taken into view, or, at all events, may be so explained as not to form any solid objections.

The sources of failure we shall endeavour to indicate, and which are thought mainly, though not altogether, to

proceed from the use of impure matter, derived from a pustule vitiated in the progress of the disease, or out of one originally spurious. The former may happen from the subversion of the specific action of the vesicle by lacerating it to get matter, or from its being accidentally rubbed,—and the latter by its existing in a diseased subject, some other morbid affection blending itself with the vaccine, and thus inducing such a change of action as to destroy its protective property.

Each of these positions is well established. Disturbed much in any way, the pustule takes on phlegmonous, or, perhaps, more commonly erysipelatous inflammation, which entirely supersedes the pre-existing state of things, and secretes, in place of vaccine virus, some modification of pus.

The power of certain cutaneous disorders over vaccination was early noticed by Jenner, and has since been more particularly pointed out by some other writers. “The most frequent cause,” says Bateman, “of the deterioration of the lymph, seems to be the presence of chronic cutaneous eruptions, or the concurrence of eruptive fevers, or even of other febrile diseases. The chronic cutaneous affections which sometimes impede the formation of the genuine vaccine vesicle, have been described by Dr. Jenner, under the ordinary indefinite term herpes, and tinea capitis. In the more accurate phraseology of Dr. Willan, they are herpes (including the *shingles* and vesicular ringworm) psoriasis, and impetigo (the dry and humid tetters,) the lichen, and most frequently the several varieties of porrigo, comprising the contagious eruptions.”

The itch, according to Willan, has the same influence, and we are told by Tierney, who has treated the subject with great ability, that ulcers as well as recent wounds, are also mischievous in their tendencies.

Concerning the powers of chronic eruptions in this case, we are assured, that Jenner has of late completely satisfied himself, by a series of observations, that vaccine will enter into combination with certain species of herpes, producing

a third disease of a hybridous nature, which may be indefinitely propagated by inoculation, without change of character, and which is impotent to all the purposes of preservation against variola.

As to the effect of the exanthemata, or acute eruptions, we are not so accurately informed. It seems however, that when measles, or scarlatina, and perhaps varicella, break out, the progress of the vaccine vesicle is arrested till these fevers abate, when it again resumes, and finishes its progress, with a retention of all its peculiar properties.

But matter originally pure may by keeping undergo some change which alters or destroys its qualities. Time, which alone will cause such effects, is much aided by a high degree of temperature. During winter the pellucid virus, which is much more perishable than the scab, may be preserved for many months; whereas, in summer, it loses its strength in a few days, and in some instances, even in a few hours. Being only impaired, it causes a pustule so imitative of the genuine one, as hardly to be discriminated—(the only particular indeed, in which the two cases differ, is, that in the former, the scab is said prematurely to fall off,) affecting the constitution however, so slightly, that no protection is afforded. An account is given by Baron Humboldt, of a surgeon of Lima, having vaccinated a number of persons with superannuated matter, brought to that city, all of whom apparently did well, though the whole subsequently received variola by inoculation, in a very mild shape. To the same point, cases might be cited, from various records, showing that virus enfeebled by age or otherwise will excite a simulated affection, which, while it is incapable of an entire resistance, tempers the violence of small-pox.

An effect not altogether dissimilar, is said to follow the use of virus from an immature vesicle. Thus we are expressly told by Willan, that if lymph be taken from a perfect vesicle too early, that is on the *sixth* or *seventh* day,

it often proves totally inefficient, and where it does operate, the genuine vesicle is not produced.*

Nor less clearly does it appear, that the genuine pustule may be local, extending no security whatever to the system at large, and thus constituting another source of failure. Not the least striking fact of this nature, came within our own observation so early as the year 1804. By the late Dr. Stewart, then physician to the Dispensary of this city, a man was vaccinated, who seemingly having the genuine disease, matter was taken from his pustule, with which several of his children were successfully infected. After a while, the father broke out with the natural small-pox, and had it pretty severely—the children however escaped, and resisted repeated variolation as well as vaccination. In the 5th volume of the Medical and Physical Journal, a case precisely similar is related by Dr. Harrison, and I have no doubt that others are recorded.† This last, we suspect, to be a common cause of miscarriage in vaccination, and so it seems to be considered by some of the best practitioners abroad. It early engaged the attention of Mr. Pearson and Mr. Bryce, by whom the disease has been most carefully watched, and they have suggested the means by which the danger from this source, in their opinion, may be obviated.

It is said by the former that if a second vaccination be performed on the sixth or seventh day after the first, a vesicle will arise which proceeds in the usual mode, till the

* There is in this respect, an analogy to small-pox. It is related by Mudge, that a woman was inoculated for this disease, and that five days after, thirty men were inoculated from her arm before the matter was matured—while in ten the operation was delayed till complete maturation. The latter had the disease, whereas, in the former there was only a local affection, and on a subsequent inoculation, the whole took the small-pox.

† The small-pox, it would appear, is liable to failure in the same way. It is indeed affirmed by Dr. Harrison that he has met with several such cases, in his own practice, or which have been reported to him, by his medical friends, and that others are to be found recorded. We too have read of similar instances. It is somewhere related by Dr. Wright of Jamaica, that with matter procured from a pustule on a negro man, he inoculated a number of slaves, all of whom had the disease so perfectly, as to resist every attempt at reinfection, though the individual from whom the matter was derived subsequently took it, in its most malignant form.

areola takes place around the primary one, when the second fades and perishes, demonstrating the constitutional affection.

By Mr. Bryce, the effect of vaccination was tried during every period of the progress of the vesicle. He remarked, that when the first operation succeeded, the inflammation excited by the second was accelerated—and as soon as the primary vesicle acquired the areola, the second, however small it might be, also had a proportional areola, and both desiccated together. He draws the same conclusion, that it is a test on which we may rely of the system being properly secured against small-pox.

Yet, whatever may be the value of this expedient, it does not appear to have been generally adopted. It is now the practice of the vaccinists of Great Britain more particularly, to proceed on the supposition, that security is best attained by the multiplication of cotemporaneous vesicles.

“As a general rule,” says our author, “it may be advisable to make two punctures in each arm, and when this is properly done, three vesicles at least, will commonly arise, and if four are excited it is never to be regretted. If only two vesicles arise, neither should be opened or disturbed.—and if the vaccine proceeds regularly to the end, the vaccination may be considered complete. When three or more vesicles have been excited, lymph may be taken from the subject. But it is prudent always to leave two complete vesicles to pass through their course untouched.”*

Though emanating from such high authority, we confess that we do not approve of this practice. It seems to us, to have originated in pathological views altogether false, and is without the sanction of any adequate experience of its utility. The system loses its susceptibility to small-pox, not by the quantity of vaccine virus introduced, but by the impression it creates, and to do which, provided it acts, one particle is as effectual as ten thousand. To believe indeed, that the living body is capable of saturation by an excess of matter, as happens in a chemical process, which

* These are the directions also of the London Vaccine Establishment.

seems to us to be the foundation of this creed, is a conclusion drawn from a very remote analogy, and in itself is futile and absurd. As a criterion of constitutional affection in this case, we are apprehensive that we are destitute of any, deserving of entire confidence, and that such an attainment is still ardently to be desiderated.

Notwithstanding this, and perhaps some other objections, it follows, we think, on the whole, from what has been said, that the common allegations against vaccination are not well sustained, proceeding rather from the carelessness of the practitioner than the demerits or imperfections of the expedient.* By due attention, we are persuaded that all difficulties may be overcome, all cavils silenced, and all prejudices removed on the subject. Considering how much our pride is interested in a discovery which so gloriously illustrates the character of the profession, we ought not to permit it to fall, or be injured, by negligence on our parts. By adverting to the loose and indiscriminate manner in which vaccination has been practised by heads of families, by the clergy, by old women, and other benevolent, though very incompetent personages, we ought rather be surprised, that the number of failures is so small. Every good is interwoven with some portion of evil. The singular mildness of vaccination, by taking it out of professional hands, has undoubtedly very much detracted from its utility, and exposed it to some unjust imputations.

Of the alleged failures, some of the cases, and perhaps many, were not really so. It is admitted, that varicella may be sometimes confounded with small-pox. By Willan, who was, above all men, skilled in the diagnosis of this order of diseases, it is declared, that in six years he saw seventy-four cases of chicken-pox, which had been mistaken for small-pox after vaccination.

* In proof of this we have the strong fact that no instance of failure has occurred in the practice of Dr. Jenner or his nephew, Mr. Jenner, and not above two or three in that of the National Vaccine Establishment of London, where vaccination is done to an enormous extent. Can any other reason be required to induce the public to confide exclusively the practice to the skilful and experienced?

Conceding, however, to the fullest extent claimed by its opponents, the failure of vaccination, the instances are probably not more numerous than of variolation. Nearly three thousand cases of the latter have already been collected, and further researches would doubtless add to the catalogue. We know not exactly the relative proportion of vaccine miscarriages, though we suspect them to be less.

Whether small pox, succeeding vaccination, assumes a mitigated shape, our opportunities of observing have not been sufficient to enable us to decide. This seems to be the opinion of the European writers, to whom such cases have more frequently occurred. We are told by Sir Gilbert Blane, among the highest of medical authorities, that "The invasion and eruption, in every respect, resemble that of genuine small-pox. I have seen it attended with high fever, and a thick crowded crop of papulæ, such as precedes the most dangerous cases of the confluent kind. This runs on till the fifth day from the eruption, both days included, at which time some of the papulæ begin to be converted into small sized pustules. The disorder then abruptly stops short. On the following day the fever is found to have subsided, with a shrivelling and a dessication of the eruption, and recovery proceeds without the least danger or inconvenience. The face is marked, for some time after, with brown spots, but without pits. It should never be forgotten, that all morbid phenomena are full of varieties and exceptions. Accordingly, though the fifth day is the most common limit of this disorder, it sometimes stops short on the third; sometimes not till the sixth or seventh; and in a very few cases, it has been known to run the common course of small-pox. What forms the strong line of distinction from proper small-pox, is that, with a few exceptions, it does not proceed to maturation and secondary fever, which is the only period of danger. I am not prepared to deny that death may not have occurred in a few instances, nay, there seems sufficient evidence that it actually has; but these adverse cases are so rare, as not to form the shadow of an objection to the expediency of the general practice. A few weeks ago at a meeting of this soci-

ety,* at which forty members and visiters were present, I put the question whether any of these eminent and extensive practitioners had met with any fatal cases of this kind. Two gentlemen had each seen a single case, and two other gentlemen took occasion to say that they had each seen a case of second small-pox, both of which proved fatal. It is evident, therefore, that according to that maxim which guides mankind in the conduct of life, namely, that of acting on a general rule and average, and not on exceptions, these adverse instances ought not to have the least influence on practice, even though they were much more numerous."

We will allow that variolation is productive of the same effect, which, however, is a point by no means settled. Yet the advantages are entirely on the side of vaccination. Exclusive of various considerations by which it is recommended, too obviously to be noticed here, it is a process mild in its general character, rarely inducing unpleasant consequences, and never proves fatal. But of inoculated small-pox one in two hundred and fifty dies, according to Willan, and several distinguished English writers have made it as one in a hundred.† Distinct from such prodigious mortality, it may be objected to variolation, that it very often entails the most lamentable effects, developing scrofula and other loathsome diseases, causes the loss of sight, and is utterly destructive, by its disfigurations, of personal comeliness.

By variolation, the sources of contagion are, moreover, multiplied, each case proving a new point from which the disease may emanate, so that, though individuals were benefited by the mitigating influence of the process, the aggregate of mischief was actually increased. Calculations made by Heberden, without any reference to this question, show, that subsequently to the introduction of inoculation, *ninety-five* persons died of small-pox in London out of every thousand—whereas the average number antecedently to it, was only *seventy* in the thousand.

What sort of an evil small-pox was, even in this state of

* Medico-Chirurgical Society of London.

† One in six dies according to the best computation, of natural small-pox.

supposed amelioration, may be determined, when it is told, that it destroyed nearly fifty thousand individuals annually in the kingdom of Great Britain only, occasioning every fourth death on the bills of mortality! By vaccination, even limited as it is by vulgar prejudices, this frightful expenditure of human life has every where been greatly abridged, and in some countries, as Denmark, where it is rendered highly penal to variolate, it no longer exists at all, small-pox having become extinct.

In concluding the subject, we have to state, that there is no evidence within our knowledge, to warrant the popular notion, that vaccination is the parent of certain foul eruptions, or that in the progress of time, the susceptibility to variola may be revived in the system. Experience, so far as it extends, confutes, we have reason to believe, each of these impressions, and that like the rest of the objections to vaccination, they are gratuitous and unfounded. The attention of Willan to the first of these points, and his ample opportunity of deciding it, are well known. By him we are expressly informed, that he is not sensible that any new affections of this sort, have been generated, since the prevalence of vaccination, or that the old disorders had become more numerous or virulent. But directly the reverse as to the latter at least, the distemperatures of the skin having diminished. It is also a remarkable fact which he cites to the same purport, that in Gloucestershire, where the vaccine has existed for half a century or upwards, in which time several hundred had the disease, no such complaints are heard of, or alleged in the case.

As to the second point, inquiries carried on in Britain very extensively, prove that time exercises no influence whatever over the process, the instances of failure not being more in the old, than recent vaccinations.

On the whole, as the result of the best examination which we are capable of giving to the subject, we cannot help recommending an entire confidence in vaccination, as adequate to all the purposes which were originally professed. Time has rather accumulated and confirmed, than impaired

the evidence of its validity, and such we are authorized to say is the opinion of Jenner himself, supported by the official reports of the Board of the National Vaccine Establishment of London.

Let it be admitted that vaccination is an imposition, all experimental inquiry is futile, our senses are illusive, medical testimony a fallacy, and we must surrender ourselves up to incredulity and scepticism, vacant and unprofitable.*

* We had intended to have introduced here some notice of the varioloid disease. But have thought it best to defer our observations till we get the work of Dr. Jenner on this subject, which we are in daily expectation of receiving.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

ANATOMY.

Anatomical discovery respecting the Lymphatic Vessels.

Dr. Vincenz Fohmann, Prosector at the Anatomical Theatre at Heidelberg, announces, in the Salzburgh Medizinisch-Chirurgische Zeitung, (No. 46, June 1820) a discovery respecting the lymphatic vessels, that seems calculated to elucidate some important points in physiology.* In his researches during last winter, respecting the intimate structure of the intestines of man and inferior animals, he found, by accident, in the large intestine of a lion which he dissected, a net-work of lymphatic vessels which had not been remarked in any animal, and which on being injected with mercury, appeared to him to *inosculate directly with veins*; (ihm unmittelbar in venese uberzugehen schien.) This discovery led him to make new researches in other animals, and he found similar lymphatic vessels in other *carnivores*; namely, the *procyon lotor* and the common dog; but here he could not clearly ascertain the passage of the mercury into the veins. He repeated, in the month of April, the injection of the same lymphatics in the *phoca vitulina*, and what he could not effect by means of the *vasis chyliferist of the large intestine*, he effected perfectly by those of the small intestine; then, after having filled all the lymphatics of *this intestine*, and having pursued them to the *pancreas uselli*, he found some small *vasa efferentia*, by which the quicksilver had passed into the mesenteric vein. He repeated the injection in a second *phoca*, and found the metal pass into the same vein as in the foregoing experiment. He on this occasion put a ligature on all the mesenteric vessels, at some distance beyond the large mesenteric glands; and he found that those glands, as well as the veins, were filled with the mercury.

He has witnessed the same facts in the dog, cow, and horse, but has not yet had an opportunity of examining the state of these parts in man: but he considers that other physiologists will investigate this subject; and he makes a question whether the thoracic duct really receives all the chyliferous vessels, or

* It is a remarkable circumstance, that Professor Harless, of the University of Bonn, had formed a notion that such an anatomical structure probably existed, before this discovery of Dr. Fohmann, as he states in his work, such a structure, he thinks, being calculated to explain and reconcile the apparently opposing results of the experiments of several eminent physiologists respecting absorption.

whether the names of Pecquet and Mascagni have caused a supposition to be admitted as a fact.

Dr. Fohmann announces that he intends to publish, almost immediately, a work on the true disposition and development of the intestinal glands of men and inferior animals, in which the facts above stated will be more particularly developed.—*The London Med. and Phys. Journal*—Nov. 1820.

PHYSIOLOGY.

Case of premature Puberty, related by Dr. Descusset.

Marie-Augustine-Michel, a native of Pouné, department of the Seine et Oise, had the menses appear at the age of thirty months, and they have continued to recur regularly up to the present period, at which she is entering her fifty-third year, excepting during the times of pregnancy. At the age of eight years, she was four feet, four inches in height, and her breasts fully developed; since which period she has not increased in height. She was married at the age of twenty-seven, and lost her first child after a difficult labour, from which she had a prolapsus of the uterus. She has since then aborted twice, and produced eight living children, two of which occurred as twins, in the last pregnancy but one. She is of a free habit of body, of a sanguineous temperament, and has constantly enjoyed perfect health, and now, in her fifty-third year, does not experience any symptom that indicates the approach of the cessation of menstruation.—*Nouveau Journal de Medecine*, tome vii.

Menstrual—Fœtal blood, &c.

Dr. F. Lavagna has been led to conclude from some experiments on the menstrual blood, that it differs from pure blood only in the want of fibrin. He also found that blood collected from the vessels of the funis umbilicalis relating immediately to the placenta, formed a tenacious coagulum, and contained a considerable proportion of fibrin, though it was rather more soft and gelatinous than that collected from the blood of an adult in the state of health: and that on the contrary, the blood contained in the vessels of the funis, relating to the fœtus, hardly coagulated at all, and seemed to contain a very small proportion of fibrin. This physiologist is induced to conclude from these observations, that the uterus in a gravid state, acquires the power of furnishing blood provided with fibrin which is subtracted from it by the embryo to be appropriated to its own use for the means of its growth.—*Annali Universali di Medicina di Milano*, No. 17.

SURGERY.

Case of extirpation of the Thyroid Gland, by Dr. Klein of Stutgard.

A boy, eleven years of age, deaf and dumb, and of very delicate constitution, had from his infancy, a circumscribed tumour situated on the left side of the neck, about the size of a nut, and which had gradually increased in bulk. After having been put under the care of several practitioners, he was brought to Dr. Klein. The tumour commenced on the left side, at the margin of the jaws, occupied the whole of the left side of the neck, as far as from the larynx to behind the ear, and down as low as the third rib. Vessels of the size of the fingers ran over the surface: it was moveable with difficulty, rather because of its weight than of its adherence to the parts beneath. It was a little irregular, or tuberculated, in its construction, and the pulsation of the arteries which penetrated its interior could be distinctly felt in several parts. Its removal by the knife having been determined on, the patient was laid on a table, so that his head, shoulders and legs, might be more effectually secured by assistants. On making the necessary preliminary incisions in the integuments, the veins, which were much dilated, were divided and threw out about half a pound of blood, but they were soon secured by compression with the fingers, as well as the subclavian artery. After the flaps of the integuments were dissected aside, the tumour was forcibly drawn outwards; and using sometimes the fingers, and sometimes the scalpel, or its handle, it was separated from the larynx and trachea. The operation occupied a minute and a half. But Dr. Klein was surprised to find no hæmorrhage even from the thyroid arteries, which were of course divided. On examining the patient, however, he was found to be insensible, and though every proper means was used for three quarters of an hour, it was found impossible to restore him to life. On opening the body, it was found that neither the carotid artery, the par vagum, or the jugular vein, nor the trachea, had been injured, but all the vessels of the dura mater were gorged with blood; there was a large quantity of serum in the ventricles. "I think myself authorised," says Dr. Klein, "to conclude that the patient died of apoplexy occasioned by the revolution in the circulation, which the operation had caused. The tumour itself was formed by the thyroid gland, which had suffered disorganization throughout its whole structure, excepting a small portion on the right side. It weighed two pounds, and its base was about six inches diameter in every direction. It was formed of a solid, lardaceous, reddish substance, divided into several lobules."

In regard to the cause of death in this case, we would suggest to others the consideration, whether or not, moral circumstances had some influence in causing it. The agitation and suffering of the boy, being deaf and dumb, and consequently incapable of understanding the object or necessity of the operation, must have been extremely intense. We doubt whether such an operation on such a subject was justifiable.—*London Med. and Phys. Journal*, for February, 1821.

It has been always known, both to physicians and surgeons, that fomentation or the application of moisture at a certain temperature, had great efficacy in allaying pain and abating inflammation; but hitherto no means have been devised for the continued employment of this remedy. An apparatus for this purpose has been lately contrived by Dr. Macartney, of Dublin, which promises to produce the most beneficial effects, even in recent wounds of the worst kind. In a case of punctured and lacerated wound in which the palmar fascia was penetrated and partially torn out, the hand was placed in the vapour of water at 97 degrees Fahrenheit *immediately* on receiving the injury, and detained in it during twelve hours, without intermission. By these means all pain and inflammation were prevented. Lint, wet with water, was subsequently applied; the wounds speedily healed without suppuration; the surface gradually closed, instead of filling up with granulations, and the cicatrices left are of the best possible kind.—*London Med. Repository*, for January, 1821.

Mr. Curtis, we have been informed, has adopted the practice of inflating the Eustachian tube, and injecting warm water into it, in some cases of deafness, with considerable advantage. The instrument he uses is an improvement of that of Guyot, as described by Garengnot; the principle of which, we believe, is employing tubes of caoutchouc instead of inelastic ones. Mr. Curtis considers that contraction of the Eustachian tube, or obstruction of it by mucous secretion, is not an unfrequent cause of deafness. He has not found the introduction of an elastic tube into the orifice of the Eustachian duct difficult in general; and if the instrument be passed rather firmly along the parts in the introduction of it, it does not produce pain or other inconvenience by its irritation.—*London Med. and Phys. Journal*, for February, 1820.

An instance of successful application of a ligature to the subclavian artery, has occurred in the practice of Mr. Liston, of Edinburgh. It was employed for the cure of an aneurism in the axillary portion of the left brachial artery. An incision

was made along the edge of the clavicle, and another, perpendicular to it, along the outer margin of the sterno-mastoid muscle; the flaps were dissected back, the internal jugular vein was divided, as were the supra-scapular artery, and part of the omo hyoid muscle. Continuing the dissection, the axillary plexus of nerves was denuded. On discovering the artery, it was found to be unsound in the part exposed; about half the scalenus anticus muscle was cautiously cut through; and, in order to avoid the phrenic nerve, the muscle was burrowed under, and its fibres cut from below upwards, and thus the artery was exposed in a point nearer to the heart. It was tied here. The ligature was tightened upon the artery by means of a small wire instrument, made like a curved fork, with an eye for holding the ligature, at each point, after the ligature was placed upon the artery. The points of the instrument were sufficiently wide apart to receive the artery between them. The needle used was made of flexible iron, inserted in a handle, with an eye close to the point which was as sharp as possible short of the point being a cutting edge. Some hæmorrhage occurred, after the operation, from the upper cut extremity of the external jugular vein; but this did not prevent the patient's final recovery.—*Proëmium to vol. 44, of the London Med. and Phys. Journal, 1st July, 1820.*

THEORY AND PRACTICE OF MEDICINE.

A new hypothesis of the cause of Diabetes. By HUFELAND.

It is observed, that diabetes mellitus is progressively increasing in frequency, and in the same proportion, calculus is becoming less common. This latter circumstance has been attributed, and I believe with propriety, to the more general use of diuretic drinks, especially of tea and coffee. Now the production of saccharine matter in the kidneys, seems to augment in the same ratio as stone diminishes. Does not this phenomenon indicate an analogy between the causes as well as the nature of these two diseases? and may not the production of sugar in diabetes be a modification of lithic production? We find in both diseases a similar fundamental condition, an abundance of free uric acid: the only difference is, that in one of these affections, the acid unites with mucus, or with earthy particles, to form a calculus, and that in the other, the acid unites with some other substance so as to form sugar. An important circumstance in support of this notion is, that chemical analysis shows, that uric acid, which constantly exists in the urine of a person in the state of health, is not found in that of a person having diabetes. Consequently, after all the fruitless attempts which have been made to cure diabetes, it remains to be examined, whether we should not treat this affection as we treat

calculous disorders; and if the same lithontriptics, which are evidently efficacious in stone and gravel, such as potash, soda, soap, lime-water, and especially the waters of Carlsbad, may not be equally efficacious against diabetes mellitus.—*London Med. and Phys. Journal.*

Marsh Miasmata, &c.

Dr. Ferguson, Inspector of Hospitals, read a paper before the Royal Society of Edinburgh, in which he endeavoured to prove, by a reference to the medical topography of various places in the south of Europe, and the West Indies, that the universally received opinions of aqueous and vegetable putrefaction, single or combined, being the sources of this poison, were unfounded—that putrefaction under any shape had no effect in producing it; that it never emanates from water in bulk, however putrid that water may be, nor is it necessarily an exhalation from marshes: but some modification of the state of the atmosphere by heat and moisture, being the product of an highly advanced stage of the drying process in absorbent soils, that had previously and recently been saturated by water. The illustrations were principally taken from the countries where the author had served with the British Armies during the last twenty-five years, and exhibited a great variety of observations in support of the opinions the paper professed to advocate. Other properties of the marsh poison, such as its particular adherence to, and attraction for, lofty unbrageous trees and rising grounds, in the vicinity of swamps; its concentration in ravines, hollows, or leeward localities; its absorption from passing over water, and rarefaction or dissipation by the sun's heat and regular currents of wind, were also pointed out and illustrated. In the course of the paper, the author, while treating of the effects of marsh poison, was led to consider its extreme and most baleful product—the yellow fever of the Tropics, and he endeavoured to show the non-contagious nature of this disease, by a series of forcible arguments. It concluded with some observations on the mode of reception of the marsh poison into the human constitution, whether by the lungs, the stomach, or skin, which last the author seems to think is the most probable channel, and he supported his opinion by some illustrations taken from the plague of the Levant; the peculiar idiosyncrasy of the African or Creole negroes, &c.—*London Med. & Phys. Journal, No. 255, for May, 1820.*

Tic Douloureux.

This most painful of all affections, has recently been treated with great success by Mr. Benj. Hutchinson, an eminent surgeon of London. The remedy in which he chiefly confides is

the carbonate of iron exhibited in the large dose of two scruples or a drachm, three times a day, to be continued for several weeks. In some instances the disease seems to be connected with a plethoric habit, and even active inflammation. This state of things is to be previously reduced by blood-letting general and topical, and other evacuations.—*Ib.*

It is stated by Mr. Charles Haden, an English surgeon, as the result of multiplied trials of the article, that colchicum united with a saline purgative, is peculiarly well adapted to the treatment of all cases of high excitement, whether acute or chronic. He insists, that in the reduction of vascular action, it is to be preferred even to the lancet. The powder is represented to answer much better than the tincture, his prescription consisting of from two to eight grains of it and a scruple or more of the sulphat of potash, every five or six hours till purging is induced. In chronic cases he gives in the morning, one large dose only, of five grains and upwards, with a drachm of the sulphat of potash in a tumbler of water, continuing the remedy for weeks together, occasionally aiding its operation by jalap or some other cathartic when necessary. The above account is fully confirmed by Mr. Rice, another surgeon, who, it appears, has administered the medicine with success, in gout and rheumatism, in active hæmorrhage, puerperal fever, erysipelas, &c. Notwithstanding these representations, we cannot help suspecting, as in the case of digitalis, which a few years ago was recommended, with the same view precisely, and in a tone of equal confidence, that expectations thus excited will be disappointed.—*Ed.*

Officers of the Academy of Medicine of Philadelphia for 1821.

<i>President,</i>	<i>Secretary,</i>
N. Chapman, M. D.	John Barnes, M. D.
<i>Vice Presidents,</i>	<i>Treasurer,</i>
W. P. Dewees, M. D.	Thomas Harris, M. D.
Samuel Jackson, M. D.	

THE PHILADELPHIA MEDICAL SOCIETY.

OFFICERS OF 1821-22.

<i>President</i> —Wm. P. Dewees, M. D.
<i>Vice-Presidents</i> —Joseph Parrish, M. D. John Barnes, M. D.
<i>Orator</i> —N. Chapman, M. D.
<i>Corresponding Secretaries</i> —
John Eberle, M. D. Samuel Jackson, M. D.
<i>Treasurer</i> —Samuel Stewart, M. D.
<i>Librarian</i> —George Treese, M. D.
<i>Curators</i> —Samuel M. Fox, George Treese, M. D.

The following gentlemen were elected as honorary members.

Dr. John Stitt,	Gilbert Flagler, M. D. Pa.
Thos. D. Mitchell, M. D. Pa.	James Cornick, M. D. Surgeon
Sir James Wylie, M. D. Russia.	U. S. Navy.
James Leighton, M. D. do.	Abraham Creighton, M. D.
Ellis C. Harlan, M. D. Pa.	Russia.
Gideon Humphrey, M. D. Pa.	

The following Students of Medicine were admitted as junior members.

Maine—John Hubbard.

New-York—Seth Salisbury.

New-Jersey.

John Elkinton,	John Brick,
Henry Holcombe,	Richard Dobbins.

Pennsylvania.

Benjamin Ellis,	Benjamin S. Budd,
Theophilus Dunn,	John Purves,
Gust. Colhoun,	Jacob Jeanes,
Nicholas B. Lane,	Wm. Davis,
Wilson Jewell,	Matthew Pryor,
Benjamin Sanford, jr.	B. Rush Rheas,
F. J. Le Moyne,	Chas. Marckley,
Wm M. Fahnestock,	Eliphalet Stevens,
Francis Waln,	Wm. Milnor.
J. Rodman Paul,	

Delaware—Bolitha Laws.

Maryland.

William Plater,	Wm. P. Williams.
-----------------	------------------

Virginia.

Wm. W. Wright,	Southey Satchell,
J. C. Taliaferro,	George Morton,
Wm. A. Lacy,	John Kerr,
Geo. W. Perkinson,	Theodoric A. Carter,
Thos. H. Averett,	G. Marion Lewis,
Edmund Berkley,	James Cooke.
Christopher Terrell,	

North Carolina.

Josiah C. Skinner,	P. H. Thomas,
Smith Murphy,	Thos. L. Carthy,
John Jackson,	Levin B. Lane.
M. W. B. Armstrong,	

South Carolina.

Thomas Smith,	S. Blanding.
Thos. A. Mc Iver,	

Ohio—Jeremiah Brooks.

Georgia.

Cullen Locket,
Alexander Jones,
A. F. Hill,
John Jordan,
Richard Banks,

Joel Branham,
J. D. Swift,
J. D. Jarrett,
Willis Greene,
Wm. Raney.

UNIVERSITY OF PENNSYLVANIA.

At a public commencement, held on the fifth of April, 1821, the following gentlemen received the degree of *Doctor of Medicine*, viz.

New Hampshire.

Jonas Underwood, on Cutaneous Sympathy.

Connecticut.

George Marvin, Periodic Headach.

New York.

Francis S. Beattie, Tetanus. Theophilus C. Dunn, Tetanus.

New Jersey.

George Haines, Cataract. Okey H. Costill, Chlorosis.
Thomas Page, Dysentery. Henry Holcombe, Marasmus.
Thos. Jones Van Meter, Can- John Brick, Traumatic Hæ-
ker. morrhage.

Pennsylvania.

James M. Staughton, Mania à John Russell, Dysentery.
potu. James R. Speer, Bilious Colic.
Benjamin Rush Rees, Yellow Ezekiel C. Cook, Cholera In-
Fever. fantum.
Thomas McEuen, Injuries of Samuel Freedley, Hepatitis.
the head. Richard Wilson, Tic Doulou-
Edwin P. Atlee, Dysentery, reux.
Jacob Hittel, Dropsy. James Hunter Ewing, Amau-
Joseph Griffith, Dysentery. rosis.

Maryland.

William Plater, Phlegmasia Christopher Stoddard, Dysen-
dolens. tery.
Stephen White, Dysentery. James A. Shorb, Menstrua-
William L. Jones, Epilepsy. tion.
Wm. Gray, Phlegmasia dolens.

Virginia.

Rich. J. McKim Holiday, Fun- John Hendree, Puerperal Fe-
gus of Antrum Maxillaire. ver.
Rich. Short, Cholera Infantum. Wm. M. Maxwell, Gastritis.

Charles C. Byrd, Secale Cornutum.	Edwd. P. Scott, Menstruation.
Charles Sturdivant, Amenorrhœa.	Isaac Telfair, Hæmorrhoids.
Jones O. Christian, Dysentery.	Landon C. Rives, Oil of Turpentine as a remedy.
James Somerville, Experiments on Absorption.	L. G. Coleman, Cynanche Trachealis.
Samuel C. Snyder, Cynanche Trachealis.	Wyatt Christian, Peritonitis.
H. W. Bramham, Dyspepsia.	George Claiborne, Bilious Disease of King William County.
Henry E. Shore, Dysentery.	Richard W. Royster, Leucorrhœa.
Thomas Old, Dysentery.	Theodorick A. Carter, Atrophia Ablactatorium.
Robt. E. Taylor, Cynanche Trachealis.	William Stith, Digestion.
Raleigh T. Colston, Menstruation.	Robert H. Cabell, Pleurisy.

North Carolina.

Jos'h C. Skinner, Hydrothorax.	John S. Jackson, Scirrhus-Cancer of Uterus.
L. H. Coleman, Typhus.	Isaac Pipkin, Phthisis Pulmonalis.
Isaac N. Jones, Typhus.	
Wm. W. Stewart, Pneumonia Typhoides.	

South Carolina.

Edward Cuthbert, Hysteritis.	Thomas Smith, Enlargement of Prostate Gland.
Wm. S. Price, Dysentery.	William F. Lee, Dyspepsia.

Georgia.

Richard Banks, Hydrocele.	Cullen Lockett, Hepatitis.
---------------------------	----------------------------

Tennessee.

William W. Lee, On Fever.

Missouri.

George P. Todson, Chorea Santi Viti.

At the same time, the following gentlemen received the degree of *Master of Pharmacy*.

John Hart, No. 8, South Second street,
 John White, No. 82, North Front street.
 John Y. Bryant, No. 27, Market street.
 Robert Milnor, No. 161, South Second street.
 Anthony Ecky, No. 53, South Seventh street.
 Thomas Cave, No. 45, Market street,
 Charles Marshall, jr. No. 310, Market street.
 Christopher Marshall, No. 19, North Fourth street.

Mordecai Y. Bryant, No. 241, North Second street.
 Mordecai L. Gordon, No. 58, North Second street.
 James D. Rooney, S. W. corner Green and Second streets.
 Charles Treichel, No. 99, Walnut street.
 George F. Garretson, corner Race and Fourth streets.
 David B. Ayres, No. 164, North Third street.
 John Stitt, corner Race and Sixth streets.
 William Poole, No. 62, North Second street.

J. R. COXE, *Dean of the Faculty.*

At a meeting of the Trustees of the Philadelphia College of Apothecaries, the following appointments were made:

Samuel Jackson, M. D. Professor of Materia Medica and Pharmacy.

Gerard Troost, M. D. Professor of Pharmaceutical Chemistry.

Annual Medical Commencement in the College of Physicians and Surgeons of the University of the state of New-York, April 3, 1821.

Under the authority of the Honourable the Regents of the University, the degree of Doctor of Medicine was conferred by the Vice-President, Dr. Post, on the following gentlemen:

Solomon A. Arnold, of Rhode Island, on the *Pestis Tropicus*.
 Gerard Bancker, of New-York, on *Diseases of the Liver*.
 Hersey Baylies, of New-York, on *Lithotomy*.
 William A. L. Collins, of Georgia, on *Aneurism*.
 James Demarest, of New-Jersey, on *Hydrocephalus*.
 Thomas J. Eppes, of Virginia, on *Anasarca*.
 Ralph C. Elliott, A. B. of South Carolina, on *Cold Bathing*.
 Edward H. Fisher, of South Carolina, on *Dropsy*.
 Wilson Foulke, of Tennessee, on *Diseases of the Liver*.
 Robert L. Green, A. B. of South Carolina, on *Syphilis*.
 Robert Greenhow, of Virginia, on *Galvanism*.
 Lana J. Hancock, of South Carolina, on *Influenza*.
 William Hume, of S. Carolina, on the *Functions of the Lungs*.
 William Ley, A. B. of South Carolina, on *Phthisis Pulmonalis*.
 Thomas L. Lamar, of Georgia, on *Uterine Hæmorrhage*.
 Lueco Mitchell, of North Carolina, on *Dyspepsia*.
 Elijah Mead, of Massachusetts, on the *Spirea Tomentosa*.
 Alexander M. Montgomery, of New-Jersey, on *Scurvy*.
 James McFarlane, of S. Carolina, on the *Fourth Stage of Labour*.
 John Neilson, junr. A. B. of New-York, on *Mania*.
 Richard Pennell, of New-York, on the *Bilious Remittent Fever*.

J. Smyth Rogers, A. B. of New-York, on *Dyspepsia*.
 Jacob Schmidt, of South Carolina, on *Sulphur Sublimatum*.
 Jacob I. Swann, of Virginia, on the *Diseases of Females*.
 Samuel T. Treat, of New-York, on *Cruritis*.
 Henry A. Tatum, of Virginia, on *Dysentery*.
 John Allen Taylor, of New-Jersey, on *Trachitis*.
 Abraham D. Wilson, A. B. of New-York, on *Hydrocephalus*.
 Robert C. Wood, of Rhode Island, on *Hereditary Diseases*.
 Reuben C. Worthington, A. B. of S. Carolina, on *Cantharides*.

UNIVERSITY OF MARYLAND.

The following graduates received the degree of Doctor of Physic in this Institution, April 2, 1821.

Virginia.

*Ed. C. Alexander, De Dysenteria. John Morgan, Fever.

Maryland.

Lloyd Dorsey, Digestion.	George A. Barber, Pneumonia.
Ephraim Bell, Typhus.	Samuel K. Handy, Croup.
Abraham Street, Physiology.	Francis Neale, Hydrocep. Inter.
Jno. P. M'Kenzie, Meloe Vesii.	Benjamin Day, Gastritis.
Abraham Jessop, Hæmoptysis.	Joseph Elbert, Apoplexy.
Richard W. Davis, Typhus.	Wm. Hammond, Cyna. Trach.
Wm. M. B. Wilson, Dyspepsia.	Richard G. Belt, Amenorrhœa.
Thos. B. Magruder, Dysentery.	Parker Forwood, Tetanus.

Delaware.

Jacob Fisher, Dysmenorrhœa. Edward K. Huffington.

Pennsylvania—John W. Davis, Gunshot Wounds.

North Carolina—Lawrence O'Bryan.

Missouri—Leo Fenwick.

Louisiana—Charles G. Lewis—Rheumatismus.

The Degree of Bachelor of Physic.

William Morgan, Delaware; Augustine Taney, Maryland;
 John Swope, Maryland; John Daily, Maryland.

JNO. B. DAVIDGE, *Dean of the Faculty.*

* Dr. Alexander obtained the prize for the Latin Thesis.

CONTENTS.

	Page
I. An Account of the Yellow or Malignant Fever, which appeared in the City of Philadelphia, in the Summer and Autumn of 1820, with some Observations on that Disease. Read before the Academy of Medicine. By Samuel Jackson, M. D. President of the Board of Health. (Continued from No. 3, and concluded.) . . .	201
II. Historical and Critical Observations on Syphilis. By A. J. L. Jourdan, M. D. Translated from the French, by R. La Roche, M. D.	221
III. On the Generation of Animal Heat. Read before the Philadelphia Academy of Medicine. By Richard Harlan, M. D. &c.	250
IV. On two new Alkalies, Cinchonine and Kinine, discovered in the Peruvian Bark. By Messrs. Pelletier and Caventon, ...	261
V. Cursory Remarks on Inverted Toe Nail. Read before the Academy of Medicine of Philadelphia. By Charles Meigs, M. D.	265
VI. On the Effects of Atmospheric Air, when applied to the Parts of the Body which are not designed by Nature to be in Contact with it. Read before the Academy of Medicine of Philadelphia. By Henry Bond, M. D. . . .	267
VII. An Essay on the Devastation of the Gums, and the Alveolar Processes. By L. Koecker, Esq. Dentist, Honorary Member of the Philadelphia Medical Society, &c. . . .	282
VIII. On the Modus Operandi of Medicines. By N. Chapman, M. D.	295
IX. An Account of the Ziziphus Vulgaris, and Ziziphus Lotus, or the Large and Little Jujubes. Communicated by William P. C. Barton, M. D. &c.	313
X. On the Tincture of the Poppy, made from the entire Plant, as a Substitute for Laudanum. By Daniel Wilson, M. D.	320

CASES.

- XI. Cases of the gradual Loss of Muscular Power, without evident Cause. By James Mease, M. D. 322
- XII. A Case of Epistaxis successfully treated by Immersion. Communicated in a Letter from James Kent Platt, M. D. Member of the College of Surgeons, London, to John B. Beck, M. D. of New-York, ... 329
- XIII. A Case of uncommon Disease. Communicated by Edward Covell, M. D. 331
- XIV. A Case of Quartan Ague, cured by repeated Emetics. Communicated by Allen M'Lane, M. D. . . . 334
- XV. Case of False Joint united by Seton. Communicated by Nicholas Worthington, M. D. in a letter to Professor Gibson, ... 337
- XVI. Case of Swelled Leg, occurring in a Male. Communicated by Henry Bond, M. D. in a Letter to the Editor, 339
- XVII. Desultory Remarks on several Cases of Disease. Communicated in a Letter from Daniel Wilson, M. D. to Richard Povall, M. D. 343
- XVIII. Observations on several Cases of Disease, &c. &c. By S. Colhoun, M. D.
- On the Thyroid Gland, ... 348
- Cases of Erysipelas, ... 351
- Cases of Exostosis, ... 353

REVIEWS.

- XIX. Report on the Epidemic Cholera Morbus, as it visited the Territories subject to the Presidency of Bengal, in the years 1817, 1818, and 1819. Drawn up by Order of the Government, under the Superintendence of the Medical Board, ... 355
- XX. The Pharmacopœia of the United States. By the Authority of the Medical Societies and Colleges, . . . 367
- XXI. *Traité de la Fievre Jaune.* Par Jean Devèze, Docteur de la Faculté de Médecine de Paris, Médecin du Château des Tuileries, &c. &c. 392
- XXII. A System of practical Nosology. By David Hosack, M. D. F. R. S. L. & E. 402
- XXIII. Experiments on the Organs of Absorption. By James Somerville, M. D. An Inaugural Thesis, . . . 408
- Obituary, ... 414
- To Readers and Correspondents, ... 415

THE
PHILADELPHIA JOURNAL

OF THE
MEDICAL AND PHYSICAL SCIENCES.

ART. I. *An Account of the Yellow or Malignant Fever, which appeared in the City of Philadelphia, in the Summer and Autumn of 1820, with some Observations on that disease. Read before the Academy of Medicine.* By SAMUEL JACKSON, M. D. President of the Board of Health.

(Continued from No. 3, and concluded.)

A PAMPHLET has lately appeared from the pen of Dr. Hosack, "Professor of the Institutes and Practice of Medicine in the University of New-York," in which the opinion of the non-contagion of yellow fever, is attempted to be controverted. Those who differ in sentiment with the learned Professor, I regret to say, are treated with little ceremony, but are denounced without hesitation, as "juniors in knowledge and in years," whose sentiments "are the unfledged opinions and speculations of men of the closet, who have had but few opportunities to test them at the bed-side," and who evince "a total ignorance of the diagnostics of the disease." When it is known, that eight out of ten of the medical profession in this country; nearly all of the practitioners in the West Indies; and a large proportion of the British surgeons, who have served in the West Indies, and become familiar with the disease—that authors of the highest repute and authority in medicine, are the persons thus treated,—astonishment must be felt at the Professor, in arrogating to himself the prerogative of infallibility. He selects Dr. Edward Nathaniel Bancroft, particularly, as the object of

his wrath, accusing him of supplying "by bold assertion, what is wanting in facts, and the coarsest personalities for dispassionate reasoning."* These grave charges could be retorted with the greatest propriety, and indelibly stamped on their author from the work under consideration. But let us hear the opinions of impartial judges. The Edinburgh Medical and Surgical Journal, in a review of Dr. Bancroft's work, expresses the following judgment of its merits.

"In the fourth part, which includes upwards of 200 pages, Dr. Bancroft has brought together such a mass of fact and testimony, all tending, in our opinion, to confirm the foregoing views (non-contagion), as has seldom if ever been collected into a focus, upon a medical topic, with so much perspicuity and force."† It is true, that Dr. Bancroft has exposed very clearly the fallacious reasoning and illogical deductions of Dr. Hosack—*hinc illæ lacrymæ*—and explains on the principles of correct philosophising, the facts that Dr. Hosack does not appear to understand. To extricate himself from the dilemma, in which he has been placed by Dr. Bancroft, he charges that gentleman with having yet "to learn the elementary truths relative to the limited and slow progress of contagion as applied to fever, and which have been known from the days of Lucretius, and have been confirmed by every writer who has treated of the *typhus fever* of Great Britain."‡ An examination of this sentence will show, that so far from shaking from himself the burden imposed by Dr. Bancroft, he has only heaped around himself new difficulties, by a great deficiency of candour and ingenuousness.

In the first place, yellow fever is not a disease of slow progress, as is insinuated, but spreads with great rapidity within the sphere of the poison by which it is produced. From one extremity to the other of the space infected, numerous cases occur in quick succession, and often simultaneously. The histories of all our epidemics are conclusive on that point, and numerous examples are furnished in the works of

* Discourse on the means of improving the Medical Police of New-York, p. 11.

† Vol. viii. p. 340.

‡ Discourse, p. 12.

Dr. Lind, to the same purport. 2dly, Lucretius does not justify the reference to his authority for the opinions or facts that the learned Professor appears to impute to him. Not a line of that acute philosopher and accurate observer can be quoted to show, that he had a belief in contagion, much less, that he makes a single allusion to the slowness of its progress. The quotations at page 30 will exhibit the opinion of the philosophic poet, as to the causes of fevers; and in his description of the Plague of Athens, he expressly attributes it to those causes.

"Hæc ratio quondam morborum et mortiferæ vis
Finibus in Cæcropiis funestos reddit agros,
Vastavitque vias." LIB. VI. l. 1137.

3dly, Dr. Hosack is a most strenuous advocate for the peculiar nature of yellow fever, considering it as a disease sui generis, and distinct from all other fevers, and consequently possessing its own peculiar laws and properties. Yet, in the passage under consideration, he unqualifiedly abandons his former favourite theories, identifies yellow fever with typhus, and would make the observations of writers on the typhus fever of Great Britain, applicable to the yellow fever of this country, and the West Indies. 4thly, It is to be presumed, that Dr. Hosack must have read Dr. Bancroft's work, and therefore he must know, that so far from having "to learn the elementary truths relative to the limited and slow progress of contagion as applied to fever," Dr. B. expressly maintains the opinion, and it is to him we are indebted, next to Dr. Haygarth, for the most conclusive proofs of the doctrine.*

An analysis of the pamphlet of Dr. Hosack, would exhibit throughout his work a similar want of ingenuousness, an equal inconsistency, and what must be considered a greater offence, in a medical writer, the same deceptious references to authors for opinions they do not entertain, and for facts that are not mentioned by them. The discourse of Dr. Hosack, having been written expressly for the instruction of students, and intended by its publication for the in-

* Bancroft on Yellow Fever, pages 498, 503, and 514 to 577.

formation of the public at large, on a subject, the correct understanding of which, is of the utmost importance to their interests and safety, a sacred regard for the accuracy of every statement, on which its inductions are founded, should have presided over its composition. As I have impeached the correctness of the "Discourse on the Medical Police of New-York," in this respect, I am bound to maintain and make good my charge, though it may not be considered necessary to enter into a very minute and critical examination of all its contents. This has in fact been done in a well-written review in the New-York Medical Repository, in which is exhibited some of the illogical reasoning and inaccurate statements of the Professor, and which very properly animadverts on the indecorous style, in which he treats those of his medical brethren, who differ from him in opinion. I shall confine myself, therefore, barely to a proof, that in some of the most important positions of Dr. Hosack, he falls under the censure I have urged against him.

1st, Inconsistency in his opinions. At page 10 we find a violent tirade against "the daring assertion made by those who acknowledge no distinction between one form of fever and another, and who can confound the *effects of heat alone* with the other causes of disease, and which," the Professor observes, "I had almost said, the youngest pupil in my hearing knows to be at variance with fact."

After this heavy denunciation of ignorance on any one "who can confound the effects of heat alone with the other causes of diseases," we are surprised to find the Professor incur his own sentence of condemnation. At page 28, we are told, that he is led to the inference, that "the *solar heat* of the tropics, long continued and acting on the northern man (not contagion) is the *exclusive source* of yellow fever."

The Doctor, at pages 10 and 11, in set phrase, is extremely severe on those who do not distinguish between common bilious and yellow fevers, and pages 26, 27, and part of 28, are occupied with facts and reasonings, tending to deny that animal effluvia can produce diseases. Yet, at page

18, he remarks, "that in those cases where *ordinary fever* is created by *animal* or marsh effluvia, the peculiarities superinduced *constituting it yellow fever*," &c. Here, then, we have an admission, that animal effluvia does produce ordinary fever, which ordinary fever can become yellow fever; the very doctrine to confute which, the discourse was written, and all who hold it are proscribed as "evincing a total ignorance of the diagnostics of disease."

2d, A deceptive reference to authors for opinions, on subjects of which they do not treat, and for facts they do not mention. This is a serious allegation, and should not be lightly made. But in a science, in which the interests of mankind are deeply concerned that truth should be established and error suppressed, when liberties of this kind are hazarded, and a serious and solemn subject, on which so much depends, is trifled with, merely to give a temporary support to a theory, it becomes, however painful the task and respectable the offender, a public duty that the practice should be exposed. It is in this spirit and with these views, and from no personal feelings, ill-will or ill-disposition to Dr. Hosack, who is a stranger to me, known only by his writings, that I am induced to the examination and exposition I have undertaken.

a. The names of Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, Blane, Chisholm, M'Gregor, Pym, Gilpin and Wright, are quoted at pages 8 and 9, in a manner to lead those who are not conversant with their writings to believe, that they have all been familiar with yellow fever, treat of it in their works, and inculcate its contagion. But what is the fact? Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, and Wright, never saw yellow fever: and their writings, with the exception of a feeble paper by Dr. Haygarth, are confined to diseases observed in England, while the only work of Sir James M'Gregor, with which I am acquainted, is on the diseases of the troops engaged in the "expedition to Egypt from India," and is chiefly occupied with the history of the plague.

The name of Huxham is of the highest repute and au-

thority in medicine. He belonged to the Hippocratic school, which, enlightened by the doctrines of early Greece, imparted to its followers the philosophic views and principles of the sages and oracles of the science of medicine. It is almost a species of profanity and sacrilege to abuse such an authority, by imposing opinions and doctrines on his works, not to be found in a page of his writings. Had Dr. Hosack perused the volumes of Huxham, he would have found principles inculcated, derived from extensive observation, and drawn from the rich stores of medical learning, entirely foreign to those attributed to him, but which are the favourite themes of the Professor. The contagious doctrine of Dr. Hosack, as applicable to epidemic diseases, it will be seen, by the following extracts from the works of Dr. Huxham, is not countenanced by that high authority.

"Epidemic diseases indeed arise from some common cause, as perhaps from a corrupted state of the atmosphere, unwholesome meat and drink, or the like, but very often, even not in the least from any *morbid effluvia that communicate the disease from the sick to the healthy.*"*

"The depraved constitutions of the atmosphere are the causes of almost all epidemic diseases."†

"As Hippocrates long since observed, and which is likewise sufficiently attested by experience, as the seasons themselves are, such also are the constitutions of the diseases."‡

"For an epidemic fever is only the effect of a morbid cause, which cause generally proceeds from some particular constitution of the air."§

b. Again, at the 11th page, we find it asserted, that "Lind, Pringle, Blane, Lempriere, Neill and Pym, have most abundantly drawn the distinguishing characters of bilious and yellow fever."

Now Sir John Pringle, so far from making a distinction between bilious and yellow fever, considers them as merely different grades of the same disease. He observes, part iii. chap. iv. of his "Observations on the diseases of the army,"

* Huxham's Works, vol. i. p. 12.

† Ib. vol. i. part 1st, preface to observations on the air.

‡ Ib. part 2d.

§ Ib. pt. 2d, xxii.

that "new-comers are liable to a more putrid and more dangerous fever, or rather to a higher degree of this bilious disorder."* And in a note to the same paragraph, he makes the following remarks. "This ingenious author (Dr. Warren) appears, however, to have mistaken the nature of the yellow fever, by referring it to the pestilential class of diseases: but though he died young, we are informed that he was sensible of his error; and, had he lived longer, would have corrected it. By Dr. Hillary's account, we can see a similarity in the symptoms, and in the treatment, with the bilious fevers of other hot climates."†

Lempriere, it is true, distinguishes between what he calls the continued endemic and endemic remittent; but he positively denies that either is contagious. He remarks, that "having given my reasons why tropical climates are not favourable to the generation or diffusion of contagion, I shall now endeavour to prove, that the disease which has been so fatal in Jamaica, did not originate in general from that cause."‡

He further remarks, "from what has been said it follows, that the disease originates in a tropical climate on shore, and that it is not contagious."§

It cannot be necessary to point out the discrepancy between these opinions, and those, that Dr. Hosack's mode of quoting authorities leads to a belief are sanctioned by them.

c. A similar inattention to correctness occurs at page 13, as will appear in the following extract. "I must, however, state, that until the facts on this subject (contagiousness of yellow fever) which have been adduced by Sir Gilbert Blane, in his well-known work, his *Diseases of Seamen*, and the body of evidence contained in the celebrated production of his later years, his *Elements of Medical Logic*, shall be disproved," &c.

The "Observations on the diseases incident to seamen,"

* P. 176, Rush's edition.

† Ibid.

‡ Practical Observations on the diseases of the army in Jamaica, p. 27

§ Ib. p. 38

by Dr. Gilbert Blane, is unquestionably a highly valuable medical work; and it derives its character from the number of facts and observations, impartially and candidly stated, without reference to particular theories, with which it is enriched. But it is in vain that we refer to its pages for a single fact or observation to show "the contagiousness of yellow fever." On the contrary, frequent mention is made of the malignant fever of the West Indies prevailing on board the fleet: but it is always attributed to exposure to marsh effluvia on shore; and allusion is no where made to its spreading among the crew by contagion.

Thus we are informed, "that the fever that prevailed on board at this time was of the most malignant kind known in this climate, and the worst cases arose in watering, and the other necessary duties on shore, from which the men would sometimes return frantic, and die in a few hours. There was a party of soldiers on board, and as they were not called upon to perform any duties on shore, they had but little sickness in comparison of the sailors."*

Other similar passages might be quoted; but the third section of chap. 1st of part iii. treats expressly of yellow fever, but not a word is contained in it alluding to the contagiousness of the disease.

"The Elements of Medical Logic," on which Dr. Hosack so much relies, appears to have been written with no other object than to defend the theory of the contagiousness of yellow fever, and is disfigured with all the errors and prejudices of a bigoted partizan. In that part of the work which embraces a view of yellow fever, a more complete misnomer could not have been selected, than the title of the work. Positions are taken without an attempt to verify them by facts, and which are in opposition to well established and recorded facts: premises are assumed, adapted to his own particular notions, without an effort to establish them by authority; and assertions are found to occupy the place of logical deductions. Such is the logic of Sir Gilbert Blane. This paper having already extended to a much

* Part i. Book i. p. 41.

greater length than was contemplated, forbids me to enter upon a detailed exposition in support of this statement. A few remarks upon a single point may, however, be deemed not inadmissible.

Sir Gilbert Blane divides the remote causes of fevers into three classes. "One class are exhalations from the soil; a second is vitiated human effluvia, generated by the living body under circumstances of crowding, filth, &c. exemplified in jail, hospital and ship fevers; and the third consists of that disturbance of the system, occasioned by fatigue, insolation, intemperance, &c. Of these three," he observes, "the second only is found to be contagious."* The true logical mode of examining the question, from these premises, would have been, to have ascertained by facts, to which of these classes of causes yellow fever owes its origin. But this plain and obvious course is neglected, and it is assumed, that the malignant fever of the West Indies, Spain and this country, commonly called yellow fever, arises from causes of the second class. Now it has been shown in preceding observations, that yellow fever, the same malignant fever so familiar to our physicians, has been and is produced by causes of the first class. The works of the most eminent writers on tropical diseases, furnish abundant testimony to this fact. I could indeed wish no better evidence than Sir Gilbert himself has offered to the medical world, in the many excellent observations he has recorded in his "*Diseases incident to Seamen*," as matters of daily occurrence, and written when unbiassed by theories. Besides, I would ask, is it at all probable, that so accurate an observer as he has shown himself to be, could have overlooked so important and essential a feature as contagion, in a disease which was daily subject to his notice on board of the fleet and in the hospitals, in the West Indies in 1781, and where the circumstances for observing that character were so favourable? As no such observations were recorded at the time, is

* *Elements of Medical Logic*, pages 130 to 132.

it not highly presumable that no facts did occur indicating the contagious nature of yellow fever?

A single page in my opinion of the impartial and practical observations of Dr. Gilbert Blane, "physician to the fleet in the late war," is of more value to science, than volumes of the partial and party effusions of Sir Gilbert, "physician to his Royal Highness the Prince Regent."

In the same passage Dr. Hosack continues to remark, that "until the affirmative testimony contained in the writings of Dr. Chisholm, Dr. Wright, Sir James M'Gregor, Dr. Pym and Sir Joseph Gilpin, &c. shall be disproved, the negative declarations of the late writers to whom I have referred must be discredited."

Now, it so happens, that the affirmative testimony of Dr. Chisholm, with respect to the ship Hankey having given origin to the yellow fever in 1793, has been confuted in the most positive manner, by the very strongest human evidence, that can be adduced. Different individuals, wholly unconnected, all acquainted with the circumstances, contradict "the affirmative testimony of Dr. Chisholm," whose statement is chiefly dependant on information, derived from others. Mr. Paiba has publicly denied having communicated the facts, for which he is quoted as authority by Dr. Chisholm.*

There appears to have been a fatality attending the attempts of Dr. Chisholm to establish the contagion and importation of yellow fever, and a most unfortunate selection of facts for the support of his statements. While the story of the Hankey has been stripped of all credibility, his account of the General Elliot, is shown to be equally devoid of foundation. Dr. William Ferguson, in an interesting paper on the nature and origin of yellow fever, in the *Medico-Chirurgical Transactions*, makes the following statement. "I feel that I have a right to use this language in regard to Dr. Chisholm's assertion, from the following circumstances. In page 119, vol. ii. of the second edition of his work on the fever of the West Indies, he states that the

* New-York Medical Repository, vol. i. p. 463.

General Elliot, East Indiaman, imported the contagion of yellow fever into Fort Royal, Martinique, in June 1796. It so happened that I, being then on my way to join the 67th regiment in St. Domingo, was ordered on board of her to take charge of the detachments of the Buffs, 38th and 60th, she was carrying out. I acted also as surgeon to the ship's crew, and can declare, that when we landed at Martinique, there was not a single sick human being, except the ship's carpenter, who was far gone in consumption; nor had there been the smallest illness among us from the day of our sailing at Portsmouth, except a few of the slightest calentures."*

In the same paper several erroneous representations of Mr. Pym, from which he infers contagion to exist, are corrected:† and as that gentleman builds his system on the verity of Dr. Chisholm's account of the origin, of what is absurdly called the Bulam fever, they fall together.

Thus the most material part of the affirmative testimony, on which Dr. Hosack lays so much stress, has been completely disproved, and that it stands in this predicament ought to have been known to him.

The Dr. Wright mentioned in this catalogue of names, has written no other work, that is known in this city, than a History of the Walcheren Remittent. He remarks of that disease, that "not having within the limits of his practice met any conclusive proofs of the reality of contagion as a cause or effect of this fever, he shall omit it in the character."‡ How does the Professor, from this writer, obtain affirmative testimony of the contagiousness of yellow fever?

d. That animal putrefaction does not generate fevers, is a position generally conceded, as a direct inference proceeding from numerous corroborating facts of ordinary occurrence. But Dr. Hosack, disdaining to sustain his views in this respect by those vulgar aids, appeals to authorities, and those not of recent date, familiar to every student, but culled from the stores of venerable antiquity, to which few

* *Medico-Chirurgical Transactions*, vol. viii. pt. i. p. 126.

† Pages 120, 121, 122.

‡ Page 8.

have access. His extensive reading and deep research are advantageously exhibited by this intimate acquaintance with authors, who, to the reproach of the profession, daily threaten to become the obsolete and dusty lumber of deserted libraries. One ill consequence, it is to be feared, attends this thirst of knowledge and devouring of books, *legendi, ac fere libris immoriendi cupiditatem*. A confusion of intellect is sometimes induced by the multiplicity of ideas, and the memory becomes loaded with such a medley garniture of facts and opinions, that nothing is perceived clearly and distinctly. “*Capacitas namque cerebri cum infinita non sit, quid mirum, quod innumerata illa idearum vestigia cerebro impressa confundantur adinvicem, mentemque à rectè et distinctè judicando prorsus avertant;*”* Dr. Hosack appears to labour under this infirmity, for it has already been shown, that he most erroneously quotes the opinions of some authors, and mistakenly refers to works for facts and principles, that are not to be found in them. The same aberrance attends his citation of authorities in proof of a point that is not contested. We are informed page 28, that, “a reference to the facts related by Diemerbroek, Rondeletius, Clavigero, Herrera, Howard, &c. will satisfy every impartial inquirer after truth, that animal matter will not generate pestilential fever.”

Diemerbroek denies that the plague has any other cause, than the just anger of heaven, awakened by the sins and crimes of the wicked. The pious and venerable author seems at a loss for expressions to testify his abhorrence of the iniquities of men, that have invited so dreadful a curse on their unhappy race. “*Prima et primaria causa est justissima summi Dei ira, quam turpissimæ ac teterrimæ exhalationes è stagnis fœdisque peccatorum nostrorum cloacis sursum elatæ provocarunt et incenderunt.*” Though such is his opinion respecting the cause of the plague, he no where adduces any facts which show, nor does he express his belief, that animal matter will not generate pestilential fevers, according to the meaning of the phrase as it is now understood. For Diemerbroek some-

* Baglivi *Præcos Medicæ*, p. 34.

times employs the word "pestilentes," as connected with the plague. He quotes in caput viii. lib. i. a fact mentioned by Paræus and Forestus, of a fever generated by a putrid whale, and also pests, cited by Augustinus and Hieronymus, as having proceeded from putrid locusts. But in caput iii. lib. ii. his opinion is still more strongly and pointedly expressed, in noticing a doctrine, then prevalent, that corrupt and putrid fetors were preservatives against the plague, as one poison sometimes breaks the force of another poison. He there positively asserts, that pestilential fevers are induced by putrid miasma. "Verum non sunt audiendi hujus fœtidæ opinionis patroni, cum putridos fœdores semper *febres pestilentes* inducere et experientia docet."* And again in the 13th section of the same chapter, he reiterates the same sentiment. "Fœtidissimam platearum, cloacarum, et fimariorum illuviem ad multorum morborum, et imprimis malignarum febrium inductionem ac pestiferi contagii propagationem plurimum facere experientia docet." These extracts are surely expressive of sentiments very dissimilar to those represented by Dr. Hosack to be found in the instructive observations of the Belgian philosopher.

Rondeletius is perfectly silent on this subject. Ferriar quotes an observation of this author from Sennertus on the incommunicability of the plague by dead bodies; but that is a very different circumstance from putrefaction not causing disorders, and has no bearing on the point in discussion. The only fact in the work of Rondeletius, having any relation to it, is opposed to the doctrine of the "Discourse on improving Medical Police," "Et, ut a nostris exempla petamus, apud Parpegnianum multa clades facta est, *pestis tum secuta est ingens*, quæ non solem vicinos, sed etiam eos, qui in montibus vivebant oppressit."† It is impossible to perceive on what grounds Rondeletius has been adduced, as affording any testimony in this case.

Clavigero is a still more unfortunate selection. I am unacquainted with any facts, contained in his History of

* Ib. Lib. ii. p. 91. † Opera Omnia Medica, de curandis febribus, p. 809.

Mexico, that can authorise the reference made to him. But he does relate facts of a widely different character from what Dr. Hosack imputes to him, and strongly conflicting with the proposition the Professor labours to demonstrate. Thus, in speaking of the condition of the Mexican capital after its capture, Clavigero informs us, "that the fetid smell, which so many thousand putrid bodies emitted, was so intolerable, that it occasioned some sickness to the General of the conquerors."

"The General caused the dead bodies to be buried, and large quantities of wood to be burned through all the city, as much in order to purify the air as to celebrate his victory."*

And again still more to the point, he remarks, "with respect to those who died by famine, or *sickness* occasioned by the brackish water which they drank, and *the infection of the air*, Cortes himself affirms, they were more than fifty thousand."†

The observations of the philanthropic Howard in his work on Prisons, will not bear the interpretation attributed to them. They may be considered as conclusively showing, that the contagion of typhus is not generated from filth; but certainly do not warrant the inference, that animal putrefaction will not occasion pestilential fever.

There is only one other discrepance between the citation of authority and the sentiments of the writer quoted, that I shall notice. In continuing the passage, on which we have remarked, the Professor observes, "and that vegetable decomposition will not engender it, no man can doubt, who will peruse the pages of Dr. Steward," &c. The words of Dr. Steward are, "that vegetable and animal matters in a state of putrefaction do produce disease, is not to be denied, but that vegetable matter only in a state of corruption is on many occasions harmless, is evident from the offensive heaps of cotton seed, &c. without being considered as a

* History of Mexico, vol. ii. p. 191.

† Ib. p. 193.

cause of fever.”* The exception has been taken as the rule—the qualification stated as the subject—a mode of reasoning that must lead to utter confusion.

Many other examples of inaccurate statements might be selected from the discourse. But it may be thought that enough has been done. Dr. Hosack’s reputation entitles him to this distinction. It is to be remembered, that he is no common authority. We have the information from himself, that his *name is great in mouths of wisest censure*, and that his opinions have been examined and “received with approbation by many distinguished practical physicians in Europe, as well as in this country.”† Errors, that in men of less notoriety, and on subjects less in vogue than yellow fever, are venial and innocent from the obscurity that surrounds them, in the distinguished and far-famed Professor swell into fictitious authority, and ripen into dogmas diffusive of many mischievous consequences. It then becomes the concern of humanity and correct philosophy, that they should be detected and exposed.

This subject, it should be recollected, has not been discussed by Dr. Hosack, as a cool and dispassionate inquirer after truth. On the contrary, he has entered the lists as an angry partisan, challenging contention, and treating his opponents with contumely and reproach. It cannot, therefore, be a matter of surprise, that such a course should raise some indignant feelings, and in a spirit of resistance, draw forth expressions, which under any other circumstances, could not be justified, and would have been carefully avoided.

* American Medical and Philosophical Register, vol. iii. p. 189

† Discourse, p. 7.

APPENDIX.

A.

It appears that the Board of Health did suppress the particular denomination of the fever reported to them by the physicians, in conformity to the Health Law. My friend, Dr. J. G. Nancrede, has furnished me with the following extract from a letter, he received from a highly respectable physician of New-York, of date January 5th, 1821. "The president of our Board of Health and our resident physician, illegally reprov'd such reports, or in other words intimidated the reporters. Several physicians, however, reported as many as twenty-eight cases of bilious malignant fever. Two of these I could name. Why the definition, in itself that of the law, was not satisfactory to them, you may clearly understand. How it happened that the simple name of fever, was substituted in the public reports, let those who held authority and paid wages for the performance of duties, answer for it."

B.

Condition of Brig Susan, Captain Smith, from St. Jago de Cuba, after unloading her cargo at Pratt's wharf.

Her cargo, consisting of coffee and sugar, with a few casks of molasses and honey, was discharged in superior order, perfectly dry, and free from any kind of damage. Having removed the dunnage, composed of pine boards and firewood, a few buckets of water only were necessary to scrub the ceiling of the sugar drippings. The brig's hold and fore-castle being unusually clean, dry and sweet, no farther purifying was found necessary.

After remaining just time enough to unlade and land her remaining stores, furniture, &c. (I think thirty-six hours), she was removed to Mr. Workman's wharf, in Southwark, where she still remains. Having turned out a West India cargo in as good order, and freer from that disagreeable smell attending confined West India produce, than I ever witnessed in any vessel during an experience of twenty years.

JAMES BELL.

C.

Health Office, August 1, 1820.

The Health Officer reports, that he has been on board of, and examined, the brig Susan, lying at Workman's wharf, in pursuance of the resolution of the Board, and finds her to be in a cleanly state; as much so as any vessel possibly can be. He further states that he has had an interview with the mate, and received from him the following information respecting the crew. Captain Smith is well; he was on board this morning. Henry Sharp, the mate, is well. A black boy, one of the sailors, has gone to sea in ship Natchez, captain Harrington. Samuel Elliot, and John Lewis, the cook, were on board yesterday, and are all well. Purnel Antrim he has not seen, nor does he know where he lives; and one man is in the Pennsylvania Hospital with a bad cough.

WILLIAM MANDRY, *Health Officer.*

D.

I do hereby certify that I was divers of times on board and in the cabin of the brig Susan, captain Smith, from St. Jago, in July last; and that a number of my friends, say between twenty and thirty, were also on board at different times, none of whom to my knowledge, did at that time, or have since, experienced any indisposition.

E. VANSYCKEL.

Philadelphia, December 8, 1820.

I do hereby certify that I had eight men, together with myself, employed two days in discharging and cleansing the hold of the brig Susan, captain Smith, from St. Jago, laden with coffee, &c. in July last, none of whom have experienced any indisposition since that time, but have enjoyed the most perfect health.

RICHARD BERRY, *Stevedore.**Philadelphia, December 8, 1820.*

E.

The brig Susan arrived, and hauled in to Pratt's wharf, July 11th, 1820. The day following her sails were unbent

and put in our loft. We do not recollect how long she remained at Pratt's wharf, not more than three days, say till the 14th July. On Wednesday the 19th Mr. James Jackson came to work for us. He continued to work for us till Saturday morning following, when he was taken sick, and died on Tuesday following, 25th inst. The report in circulation respecting the manner in which he came by his death, viz. that he assisted us in removing the brig Susan's sails, and from them took the yellow fever, of which he died, is incorrect. We do testify, that he never had, to our knowledge, any thing to do with the sails. We know he had not while they were in our loft, and we do not believe he ever was on board the brig. KEEN & DRAIS.

Philadelphia, January 31, 1821.

F.

Dr. George F. Lehman, the Lazaretto physician, has furnished me with the following interrogatories he put to captain Smith, of the brig Susan, on the 2d August last, at which time he was at the Lazaretto on business; with his replies thereto.

Question. Captain Smith, how many spare sails had you on the last voyage?

Answer. One topsail only.

Q. How was that used during the voyage?

A. Outward bound it had been bent. In St. Jago we used it as a middle awning. It was up for about fifty days, exposed to all weathers, until we sailed.

Q. What was then done with it?

A. It was folded and thrown on top of my cargo in the hold.

Q. Mr. Geisse, the supercargo, died a few days after you left St. Jago. Did he use it?

A. No.—He died in the cabin, and had no connexion with it.

Q. Was any sick person near it?

A. Never.

Q. Did it lie in the hold all the passage home?

A. No. I had it on deck several times for the men to repair it.

Q. Were any of the crew sick after working on it?

A. No.

Q. Had you any complaint of sickness after the death of Mr. Geisse among your people?

A. No.

Q. Have any of your crew been sick since leaving Lazaretto?

A. No. I believe they have all gone to sea again.

Q. Captain Smith, I mean to put this statement on record.—Are you willing to swear to it?

A. Yes.

From this account it must be obvious that the spare sail could not have caused the fever. Comment is unnecessary.

F.

John Hemphill informs me the sloop Hector was owned by Mr. Bailey of Wilmington, where she arrived from Cape Henry, and discharged her cargo, and remained twelve or fifteen days. She was washed out and cleansed, and then took in a cargo of corn meal, which she discharged at Masden & Bunker's wharf. She then hauled down to the first wharf below Walnut Street. None of her crew were sick, either at Cape Henry, or on her return voyage, or afterwards, so far as he knows; and the same crew returned in her that went out in her. She arrived at Philadelphia the 22d July.

H. COPE.

10th mo. 13th, 1820.

G.

Philadelphia, August 29.

GENTLEMEN,

The present is to lodge information with you of the greatest nuisance in Philadelphia, we mean Pegg's Run, that putrid deposit of human excrements, corruptible matters of every kind, and every species of dead creatures. This, in time, will be the origin of serious consequences. Now we wish you to take into consideration that a bridge has been built in New Market Street over Pegg's Run. Whether it is owing to an error in levelling the bed

of the creek, or to the non-removal of the dirt, with which a bank was made to turn the waters, we cannot pretend to say: but what we can ascertain as fact is, that there is a pond of stagnated putrid water, a corruption to the air we breathe, a nursery for vermin, mosquitoes, gnats, &c. so as to make our houses nearly uninhabitable and unsafe for our health. We request you to take this our complaint into consideration, and exercise the power lodged with you to remove this nuisance.

We remain with respect,

Your humble servants and fellow-citizens,

ISAAC CLEAVER,	HENRY LENTZ,
JOHN W. GOODWIN,	JACOB TRIPLER,
GEORGE GORGAS,	JOSIAH NATHANS,
B. O. HODGES,	JOHN GOODMAN,
A. KLINGLE,	GEO. F. GOODMAN,
J. M'MURDLE,	WILLIAM R. BELL,
GEORGE SHADE,	WM. MITCHELL,
J. MILLER,	JAMES LOVE,
JOSEPH MARTIN, JUN.	THOMAS STEEL.

The Gentlemen of the Board of Health.

H.

Extract of a Letter received from Dr. J. Martin of the Northern Liberties.

December 16th, 1820.

SIR,

In conformity to your desire, I will state that in the Spring of the present year a bridge was built over Pegg's Run at New Market Street; that on account either of its being higher than the bed of the creek, or owing to a dam erected a few rods above it, all last Summer there existed, from Second Street, a pond, from two to three hundred feet in length, ten to fifteen wide, at least upon an average two feet deep, of the most obnoxious and stagnated waters, a collection of dead animals, and offals of all kinds, so as to render the atmosphere absolutely offensive to the whole neighbourhood.

ART. II. *Historical and Critical Observations on Syphilis.* By
A. J. L. JOURDAN, M. D. Translated from the French, by
R. LA ROCHE, M. D.

THE physician who devotes himself exclusively to practice, directs all his efforts to one object—that of preventing, as far as possible, diseases, curing or palliating them, and combating their disagreeable or deleterious consequences. He attaches little value to their origin, and disdains to trace, if we can use the expression, the geographical march they have observed in their propagation on the surface of the globe. But he who does not consider medicine merely as a profession, the most noble and honourable of all, but as an assemblage of scientific knowledge, endeavours to acquire precise notions of the history of those affections to which the human frame is subject, and especially such as are presumed to have been little known to the ancients, or are more widely spread than they were formerly.

On this point, none merits so much the attention of the physician as syphilis, whether we regard its attacking the human species in the organs of generation, or because none has inspired greater terror, or left more disagreeable reflections:—or, as expressed by Haller, since it occupies a foremost rank among the causes that brought about the reform in medicine, and the overthrow of the errors accumulated during the middle ages, by the Arabs, or their ignorant translators, copiers, commentators, or compilers.

For the last three centuries, the subject of the origin of syphilis has been much agitated, and during this period so many different opinions were advanced, that we are bewildered when we attempt their enumeration. Yet, notwithstanding their number, we are far from having arrived at a satisfactory solution of a problem no less interesting to medicine than to anthropology.

It was about the end of the fifteenth century, as is well known, that Europe was invaded by a disease which on all sides spread alarm, horror, and death, and of which it is said

the present syphilis is a modification. The first writers who described it were, however, far from considering it a new affection: they on the contrary, for the most part, viewed it as a disease known to the ancients, but having assumed an epidemical character, influenced by the atmospheric constitution of the times, and thus become a general pestilence, which, after raging about seven years with incredible fury, disappeared gradually, leaving behind phenomena analogous to those already observed by antiquity.

At that period, ignorance, the mother of credulity, caused every mind to bend under the dominion of judicial astrology: the disease was therefore attributed to the influence of the constellations, and the conjuncture of the planets. The cause was afterwards supposed, conformably to the Galenical doctrine, to consist in a certain morbid constitution of the humors, which, originating in the liver, propagated itself to the genital organs. The alimentary substances were also accused—next, the state of the atmosphere, and the inclemencies of the seasons. Finally, the erroneous theories of the times on the nature and qualities of the periodical discharge of women, led to the admission of a particular and specific virus, producing all the syphilitic symptoms, and created a confused idea of the system, which gradually has been extended.

Such was the state of things when, in the year 1518, the opinion, that the disease had been brought from America, was suddenly advanced. It originated with a German physician of the name of Leonhard Schmauss*, professor at Saltsburgh. *Compertum est jam omnibus, occidentales indos, per plurimos annos hoc morbo graviter laborasse, medicinamque quâ semper usi sunt contra hunc morbum (lignum indicum) nostris mercatoribus jam indicasse.*

This author is hence content in affirming, without however advancing any proofs or citing any authorities, that his opinion was the one generally received in his times. Ne-

* *Lucubrationeula de Morbo Gallico, et cura ejus noviter reperta cum ligno indico, in 8vo, 1518.*

vertheless, this we are warranted to doubt, since nothing similar is to be found in any other writer, either anterior or cotemporary. Schmauss also participated in opinion with some of his predecessors, and among others with the wise Leoniceno, thinking with them that the damp heat of 1494 conduced to the development of the disease.

The year following, the Chevalier Ulric Van Hutten, who afterwards became so celebrated for his zeal in the cause of Luther, his persecutions, the important part he plays in the political and literary history of the times, and for the sarcasms thrown against him by Erasmus, adopted the opinion of Schmauss. The celebrity he had acquired in the learned world for the great originality of his ideas, the nobleness of his character, the freedom of his opinion, and the elegance of his style, contributed more to its adoption and diffusion, than the insignificant authority of his obscure countryman.*

This opinion of Schmauss was, however, adopted only by a few physicians, and would in all probability have remained in oblivion, had it not pleased the Spaniard Oviedo, in the years 1525 and 1535†, to defend and propagate it, merely from motives of personal interest. We cannot believe that the works of Schmauss and of Hutten could have been known to this historian. The literary intercourse existing between Spain and the other countries of Europe, Germany especially, was at that time very limited. But personal motives, which we shall hereafter develop, led him to the idea of attributing to America the origin of syphilis. The tone of assurance which he assumed, and the favourable circumstance of his living at the time of the discovery of the New World, where he held important offices, caused his opinion to be generally received as the most plausible and true. Nicholas Massa, Leonhard Fuchs, J. B. Montanus, Vidus Vidius, Gabriel Fallopius, J. Fernelius, Francis Renner, J. Crato

* *Libellus de guaiaci medicina et Morbo Gallico*, in 4to, Moguntiz, 1519.

† *Relacion sumaria de la historia natural de las Indias*, in fol. Sevilla, 1525.
Historia natural y general de las Indias, in fol. Sevilla, 1535.

Craftheim, Peter Lowe, Hercules de Sassonia, Eustachius Rudius, John Calvo, Andre de Leon, Fabricius Hildanus, Martin Lister, and many others, adopted it. This opinion was even embraced by persons ignorant of medicine, so that in the space of fifty years, the American origin of syphilis was considered as one of the most authenticated and incontestable facts in history.

In the midst of this general belief however, some authors, and especially Van Helmont (in his *Tumulo Pestis*,) thought the disease, though a new one, was not derived from America, but had originated in Europe, appearing for the first time during the expedition of Charles VIII. against Naples. This opinion had already been advanced by Rembert Dodoëns in his *Annotationibus ad Benivenium*, and also by D. Sennertus, whose words were afterwards literally copied by Francis Calmette, of Montpelier. On the other hand, the Italian philologist Thomas Rangonus, physician at Padua, believed syphilis to have originated in Galicia, a province of Spain, and consequently gave it the name of *Malum Galecum*. In 1787 John Howard also maintained, that the disease had not been brought from America, but had first appeared in Spain.

From time to time several strange opinions were advanced, which in general were void of solidity, being built on frivolous hypotheses: some were founded on ill conducted historical researches, or supported by badly interpreted passages—and though they no sooner appeared, than they sunk into oblivion, from their singularity, they deserve now to be noticed.

In 1680, Samuel Janson, who had resided many years in the West Indies, without observing syphilis to be there endemic, supposed it to have been brought to the new world, by the slaves from Africa. He was the first to advance this extraordinary hypothesis, and from him probably it was borrowed by Sydenham. The same opinion is found in 1684 in a work by E. Blancaard, who supports it with several ill interpreted passages of the historian Herrera, and of Ferdinand Cortez. It enlisted also the great Boerhaave, by whom

it was defended with great warmth in 1751. But it is refuted by the fact, that slaves from Africa were not brought to America previous to 1503, at which time the disease was prevailing all over Europe.

Leonhard Fioravanti, the Italian alchemist and empiric, propagated a still more singular fable on the origin of this affection. The length of the siege of Naples, he says, having caused a famine among the French and Spanish troops, the merchants who brought food to the soldiers, sold them several articles prepared of human flesh, and all those who made use of this horrible aliment were soon affected with syphilis, which was disseminated by contagion through Italy, France, and Spain. What appears most extraordinary is, that Lord Bacon put faith in this strange story, and even endeavoured to render it still more plausible: *constat enim, cannibales ad occidentem vesci carne humana, eaque India primum detectat cum esset, plurimum laboravit hoc morbo Neapolitano*. Even later, in the year 1639, Peter Benoist, professor in Montpellier, maintained in an inaugural dissertation, that syphilis owes its origin to anthropophagy. This absurd hypothesis, however, needs no refutation. Fioravanti is also guilty of an historical falsehood, as the city of Naples at the time alluded to, was besieged neither by the French nor Spaniards.

Nevertheless, in the midst of the preposterous notions of these authors, the most part of which were unknown even to their cotemporaries, the origin of syphilis did not seriously attract attention. This essential part of the history of medicine had received but feeble elucidations from the labours of the celebrated Freind, who adopted the American origin of the disease, or from those of J. B. Faust Alliot, who supported its antiquity,* or from those of William Becket, who partook of the same opinion, and endeavoured to prove that syphilis is only lepra degenerated.† Finally, a man whose merits have been greatly exaggerated, but whose vast learning and great patience in his-

* Quæst. Med. An morbus antiquus syphilis—Paris 1717, affirm.

† Philosoph. Transact. No. 357, p. 839., No. 565, p. 47., No. 366, p. 108.

torical researches cannot be denied, undertook a work, which, had its erudition been aided by a more just and severe criticism, would have established his reputation on the most solid foundation. I allude to J. Astruc. Convinced before hand that syphilis had been brought from America—that it was a new disease—that it had not been propagated by the French—that its symptoms in his time differed from those it presented when first introduced, and that it had passed through several distinct periods, he wished all to be subservient to his views.

He adopted implicitly the stories of Oviedo—rejected as inaccurate and false, the observations of those who contradicted him, and when he found this impossible, attempted to weaken them by artful, and sometimes ridiculous reasonings: gave in some instances a wrong sense and meaning to the works he read—went so far as to corrupt the text of the original—and from garbled extracts drew conclusions, contrary to the sense they present when read in the order assigned them by the author—and even did not fear to commit a number of chronological errors, much less pardonable in him than in any other.*

Notwithstanding all this, his designs succeeded, and his historical romance on syphilis was universally applauded, and soon boasted as many defenders as readers. Among them may be mentioned, Samuel Schaarschmidt, Fortune, Antony Cren, Haller, Van Swieten, Robertson, Culen, Turnbull, Bertrandi, Arnemann, &c.

A less partial observer however, Antony Nunnez Ribeiro Sanchez,† far from being dazzled by the reasonings of Astruc, or borne down by the torrent, criticised, commented on, and refuted the work of his celebrated predeces-

* *De morbis venereis, libri sex.* in 4to. Paris, 1736.—The second edition enlarged with three additional books, appeared in Paris in 1740; the third, enriched with notes by Astruc himself, was printed in 1755; and the fourth in Venice in 1760. The French translation, in which the bibliographical part has been omitted, contains useful annotations by Louis (in 8vo. Paris, 1777).

† *Dissertation sur l'origine de la Maladie Venerienne, dans laquelle on prouve qu'elle n'a point, été apportée d'Amerique, et qu'elle a commence en Europe par une epidemie.* 12mo. Paris, 1752.

sor—and by means of correct historical investigations, succeeded in collecting a number of passages demonstrating, or at least authorizing the belief, that syphilis is not of American origin, and that it began epidemically in Italy, from which it was diffused over Europe. To this work Van Swieten replied with his usual erudition.* And Sanchez published a second time in defence of his opinion, presenting further testimonies in its favour.†

D. Francis Xavier Clavigero also published a dissertation, the object of which was to prove, that the venereal disease had not come from America.‡ A few years after, the same opinion was expressed by an anonymous Spanish writer:§ of his work, which is well written, he devotes thirty-four pages to prove that syphilis was not brought from St. Domingo—that for thirty years after the discovery of the New World no one attempted to trace it there—that Oviedo is the first who advanced this tale, and that malice alone suggested to him such a calumny. It would appear that many in Spain have embraced these sentiments, as is shown by the title of the book of Sarmiento,|| which has, as yet, only been known in England.¶ John Reinhold Foster, who on the authority of Sanchez, thought that syphilis had not been imported from America, was known a long time previous to the first voyage of Columbus, and could originate from a carnal intercourse between persons affected with leprosy, believed that it had not been carried by the Europeans to the inhabitants of the South Sea islands, among whom it had appeared endemically a number of years before.**

* Comment. in Boerhaavii Aphorism. Lugd. 1772, 1 v. p. 373.

† Examen historique sur l'apparition de la Maladie Venerienne en Europe, et sur la nature de cette épidémie. 12mo. Lisbonne, 1774.

‡ This dissertation may be found in the 4th volume of *La Storia antica del messico*, 1781.

§ *La America vindicata de la Calumnia de haber Sido Madre del Mal Venereo*, Madrid, 1795.

|| *Antigüedad de las bubas*, Madrid, 1787.

¶ *English Reviews*, September, 1788, p. 221.

** *Observations made during a Voyage round the World*, in 8vo. London, p. 470.

He was answered by Dr. Martin Wall, professor at Oxford *: David Samwell, surgeon to the Discovery, one of Captain Cook's vessels, endeavoured however to demonstrate that the disease existed and was spread in the Sandwich Islands previous to the arrival of that celebrated navigator. † Cook, it is true, in the narrative of his voyages, contradicts the account given by Foster, and assures us that the inhabitants of Otaheite gave it the name of *English disease*, which would seem to indicate that they derived it from them. Yet I am inclined to believe, with the learned Sprengel, ‡ that more reliance should be placed on the testimony of Foster and Samwell, for Cook, at that time, was engaged in researches which to him appeared far more important than the period of the first appearance of syphilis in the South Sea islands. He was, besides, less acquainted with the language of the country than Foster, whom he was obliged to employ as interpreter.

Notwithstanding these efforts, which made little impression, and were by prejudiced persons considered as arising only from a spirit of controversy, the infallibility of Astruc remained undoubted—and the writings of Sanchez were almost totally forgotten, when Philip Gabriel Hensler, first physician to the King of Denmark, undertook their defence, and gave to the opinions of the author the greatest degree of plausibility. § From the first part of his celebrated work, the only one published, the following conclusions may be drawn: that the syphilis of the fifteenth century was entirely different from all the diseases anteriorly known—that it first appeared epidemically—that it was accompanied with symp-

* Dissertations on select Subjects in Chemistry and Medicine, in 8vo. Oxford, 1783.

† A Narrative of Captain Cook***with Observations respecting the Introduction of the Venereal Disease into the Sandwich Islands, in 8vo. London, 1786.

‡ See his translation of P. A. Perenotti di Cigliano, entitled, *Storia generale et regionata dell' origine dell' essenza o specifica qualità dell' infezione venerea, &c.* in 12mo. Torino, 1788. Sprengel has added a number of learned notes to this work.

§ Geschichte der Lustseuche die zu Ende des xv. Jahrhunderts in Europa ausbrach in 8vo, Altona, 1788.

toms which, taken separately, were all known before—that in the present state of our knowledge it is impossible to determine accurately the precise period of its first appearance in Europe, and finally, that it was not brought from the New to the Old World. Kurt Sprengel, appreciating the force of these arguments, embraced this opinion.*

On the other hand, Christian Godefroy Gruner, physician at Jéna, decided openly in favour of the American origin; † and Christopher Girtanner defended it with a warmth indicative more of a desire to distinguish himself by adopting an opinion fallen into disrepute, than that of combating in the cause of truth ‡. Master of many languages, and having at his disposal one of the richest libraries of the world, Girtanner spared neither care nor research—and if he supported a bad cause, he is at least entitled to the credit of having contributed greatly in the elucidation of many obscure points, and of exciting in a greater degree the attention of the learned to the investigation of a subject hitherto very superficially examined. It is to be regretted that he, as well as his predecessor Astruc, is accused of unlimited partiality to his system, and of injudicious criticism. Hensler, whom he had attacked directly in this book, answered him in a polemical work, in which decency of manner is blended with the most profound reasonings. § Girtanner again endeavoured to defend himself—and for want of valid argument had recourse to invectives. || Gruner, who at first had embraced the opinion of Astruc, gradually abandoned it as his reflections became matured by age, and finally contended that the disease had originated in Europe. ¶

* See the translation before cited; and also the second volume of his *History of Medicine*.

† *Morborum Antiquitates*, in 8vo. Vratslavizæ, 1774.

‡ *Abhandlung ueber die venerische Krankheit*, in 8vo, Göttingen, 1788—1789.

§ *Ueber den Westindischen ursprung der Lustseuche*, in 8vo, Hamburg, 1789.

|| *Intelligenzblatt der allgemeinen litteratur zeitung*, 1789, No. 182, s. 617.

¶ *Aphrodisiacus, sive de lue venerea*, in fol. Jenæ, 1789. *De Morbo Gallico Scriptores Medici, et historici partim rari, partim inediti*, in 8vo, Jenæ, 1793. *Spicilegium* 1. viii. *Scriptorum de morbo Gallico*. in 4to, Jenæ, 1799—1800.

Such is the historical sketch of the different opinions advanced on the origin of the venereal disease. It is evident they may all be reduced to three:

1. That syphilis has prevailed from the earliest periods of human existence.

2. That it has been brought from America.

3. That it has originated in Europe.

And, according to a fourth, advanced within a few years past, that it does not exist as a simple disease, but is the result of several distinct affections, between which an imaginary connexion has been established.

In the following chapters I shall detail these opinions in a more particular manner, and point out all the arguments alleged for and against them.

Hensler, Sprengel, and Gruner are my guides in this undertaking, which will be, in fact, a mere exposition of the researches to which these learned writers have devoted themselves, and which they have been led to publish in a controversial form. This prevents us from appreciating their real value, at the same time that it renders the reading of them very fatiguing to those unacquainted with the disputes excited in Germany on the subject of the history of syphilis.

CHAPTER II.

A reflection that naturally offers itself when we meditate with impartiality on the history of syphilis is, that excesses in the pleasures of Venus, as in every thing else, must at all times have been attended with unpleasant results—and that it is improbable Providence should have deferred, till the time of the discovery of the New World, the infliction of a just punishment on debauchery and obscenity. The partisans of the American origin of syphilis have not as yet been able to refute so natural a suggestion. In another place I shall make known the subtle distinctions

they have imagined for the purpose of reconciling it with their very favourite hypothesis. This last I intend here to develop, and, as Girtanner is its most recent defender, will state his arguments, and next combat them.

Girtanner maintained that syphilis did not appear in Europe previous to the first return of Christopher Columbus, and that the disease broke out in the four places where the Admiral and his men had landed, namely, at Lisbon, Seville, Barcelona, and in Galicia, though first at Barcelona. To these assertions he adds, that the disease was widely spread in the West Indies, where it affected most of the natives—that it was almost equally so on the continent, but did not rage there with so much violence as it has since done in Europe—that among the Americans it appears to have been a simple cutaneous affection, a species of itch—that the Spaniards had not the least idea of this malady previous to their first voyage to the New World—that they received it in that country during their intercourse with the savage women—that by them it was brought to Spain, where it soon made rapid progress—that a great number of those who had caught it, either in America or in Spain, followed Gonzalvo to Italy, and there communicated the virus to the prostitutes of Calabria and Naples—and, finally, that these infected the French, who afterwards spread it over all Italy, and also in France, from whence it was extended to the rest of Europe.

Before entering into any discussion, I think it indispensable to ascertain accurately the precise time at which syphilis manifested itself in Europe. This being done, less difficulty will be experienced in determining in what portion of this section of the world it primarily appeared.

All the historians who wrote either in the beginning of the sixteenth or end of the fifteenth centuries, concur in considering the epoch of the expedition of Charles VIII. against Naples as that of the occurrence of the disease. I will, therefore, for the purpose of facilitating the understanding of the following details, give a brief exposition of the political state of Europe at that time, and point

out the dates of the divers events, which will serve as a basis to our ultimate researches.

Ferdinand V. surnamed the Catholick, and Isabella, were, towards the end of the fifteenth century, seated on the thrones of Spain and Sicily. This last country had fallen under their power after the death of Peter III. King of Arragon, who had taken possession of it by causing all the French to be slaughtered. This massacre is well known in history under the name of Sicilian Vespers. Naples at the time was ruled by Ferdinand II. Charles VIII. King of France, since the death of Louis XI. was contending for the counties of Roussillon and Cerdagne, with the King of Spain. At last, the dispute between the two monarchs having terminated in an alliance offensive and defensive, concluded in 1493 at Narbonne, Charles resolved on prosecuting claims which, as heir to Charles Duke of Maine, he had to the crown of Naples, and in consequence made every preparation to attack its sovereign. He raised a powerful army, and ordered the equipment of a fleet at Genoa. On the 23d of August of the year following he left Vienna in Dauphiné, marched towards Italy, and, shortly after, his fleet set sail. Owing to indisposition, he was forced to make a stay of a month at Asti. He afterwards passed through Lombardy and Tuscany—directed his march towards Rome, and arrived in that city on the 31st of December. In February, 1495, he appeared before Naples, and made his entry into that city on the 21st of the same month. He was crowned on the 20th of May, and immediately afterwards proceeded to France with part of his troops, leaving the rest of the army under the command of the Count of Montpensier. On his arrival near *Fornoue* on the Tarro, he fell in with the combined army of the Venetians and Milanese, which, on the 6th of July, he completely routed. After this exploit, he continued his march, and the allies invested Novara, the siege of which continued from the month of August to that of October. The King of Spain, fearing that the French, encouraged by their brilliant achievements, would endeavour to regain possession of

Sicily, that had been taken from them by an act, the barbarity of which could hardly be excused by the love of independence so natural to a people jealous of its honour, declared openly in favour of the old King of Naples. He consequently proclaimed war against Charles VIII. on the 21st of January, 1496, in the city of Velitri, near Rome, and sent to the assistance of his kinsman an army commanded by Gonzalvo of Cordova. This general, rendered famous by the conquest of Grenada, arrived at Messina on the 24th of May, left there immediately for Calabria, landed at Reggio, and soon fell in with the French, who being inferior in number, were completely defeated at Semirana, and meeting with many losses, both by defection and arms, were in the end compelled, towards the close of the year 1496, to evacuate the Neapolitan territory. Ferdinand then entered his capital, as Charles had done, without meeting with the least resistance.

Such is the summary of those events the more necessary to notice, since they have been made use of in explaining the propagation of syphilis. Let us now endeavour to establish in what year, and if possible in what month, the disease was first noticed.

Baptist Fulgosi, who, after having fulfilled the high station of Doge of Genoa for five successive years, was deposed and exiled in 1483, tells us, that two years previous to the arrival of Charles in Italy, (consequently in 1492), a new disease was observed, to which the physicians could neither give a name nor oppose remedies—that it was by the Italians called *French disease*—whilst by the French it received the appellation of *Neapolitan disease*. After giving an excellent description of it, he adds that this plague (*ita enim visa est*) had been brought from Spain to Italy, and to the former place from Africa.*

E. Capreoli, a learned jurist at Brescia, though on whose accuracy in chronological observations we should place little reliance, says, that in 1494 (bienno post 1492) there appear-

* De dictis factisque memorabilibus collectio, in fol. Mediol. 1509, lib. i. cap. 4.

ed a horrid malady called *French disease*, and which, according to all accounts, invaded nearly the whole surface of the earth.*

Mark Antony Croccius Sabellico, an esteemed historian, testifies, that, from 1493 to 1494, Italy was ravaged by a new malady named *French disease*.†

Stephen Infessura tells us, that in the month of June, 1493, the Spanish ambassador expressed his astonishment that the Pope should receive in Rome the *Marranes*‡, who, as enemies to the true faith, had been expelled from Spain by the King his master. Of these unfortunate fugitives, many were encamped under the walls of the city, near the Appian gate: but some entered secretly into the suburbs, where, it is supposed, adds Infessura, they introduced the germs of a plague, which caused many deaths, and, among others, that of Cardinal Comitibus. The Pope, in April, 1494, advised Charles VIII. not to enter Rome, on account of a great plague which at that time was raging in the city.§

J. Burchardi informs us, that the 26th of July and the 11th of August, 1494, anniversaries of the death of Innocent VIII. and of the elevation of Alexander VI. were not celebrated, owing to the plague, which was then committing great ravages in Rome.||

Peter Delphini writes, on the 4th of January, 1494, to

* De Rebus Brixianorem, libri xii. in Grævii hist. Ital. t. ix. part. vii. p. 125.

† Rapsod historiarum ab urbe condita enneasdes X. Lugd. 1539, lib. ix. p. 539.

‡ The opprobrious epithet of *Marranes* (hogs) was given to the Jews and Moors, who, for having refused to be made Christians, were expelled from Spain, by an edict of King Ferdinand, dated March, 1492. To escape the horrid persecutions practised against them, they concealed their faith, and at the same time secretly practised those rules which their religion prescribed. These unfortunate victims to monkish intolerance lived in the most disgusting filth, and among them leprosy was common. Constrained to abandon their native soil, without permission to take either gold or jewels, they for the most part retired to the northern shores of Africa, where, according to Leo the African, they propagated a contagious disease. Such were the ravages of this epidemic, that of 170,000 families who crossed over to Africa, 30,000 were destroyed. I shall return to this subject in another place, for it is one of the greatest importance in the history of syphilis.

§ Diarium urbis Romæ; in Eccard. Corp. hist. Med. ævi. t. xi. p. 2012.

|| Diarium Curie Romanæ sub Alexandro VI.; in Eccard. Corp. hist. Med. ævi. t. xi. p. 2017.

Cardinal de Sienne, afterwards Pope, under the name of Pius III. advising him to be very careful in Rome, where the plague, though abated, had not entirely subsided. On the subject of the French invasion, he says, February 20th, 1494, that there is every reason to fear the passage of so many troops will serve to spread more extensively the disease, which has not yet ceased to desolate all Italy.*

Julius Seracinus, who wrote, it is true, at a much later period, remarks, that the same plague existed at Ancona in 1494.†

The four last mentioned historians, giving to this disease the name of *plague*, we might suspect them to mean some other malady than syphilis, did we not know that this last originally manifested the characters of a truly pestilential disease, attacking, as Sabellico informs us, one individual out of twenty, and occasioning death in a short time. Serenius, as quoted by Sebastian of Aquila, says, that this affection was *Lethalis etiam cita morte*. Béroald calls it *Lues pestilentia pestilentior*. It is also called *plague* by Conrad Gilini, and other physicians. The acceptation of the word plague was, at that time, far more extended, than at the present. *Est morbus multiplex et subitus uno et eodem tempore multitudini hominum eveniens*, according to the definition of John Salicet, commonly called Widman. This writer divides epidemical diseases (*Epidemiales*) into particular, when a person is affected with malignant pustules, carbuncle, &c. &c., and into general, or those that are endemical or pestilential: *non idèò quod sint plurimùm mortiferi sed idèò quod communicantur multis ad similitudinem morbi pestilentialis*. The sense of words changing sometimes with time, it becomes necessary, for the purpose of a correct understanding, to determine with precision their meaning at the different periods of the art. The word plague was applied at that time, not merely to all affections contagious in their nature, but also to those

* Epistolarum, lib. xii. in fol. Venetiis, 1524, lib. iii. ep. 84—92.

† Notizie istoriche della città d'Ancona, in fol. Roma, 1675.

rendered universal from a peculiar constitution of the atmosphere, or some other analogous causes. This application was long continued, and the word was soon made use of to denote diseases which, at the present time, we should be far from regarding as pestilential, such as coryza, itch, raphania, &c. Moreover, whilst, as mentioned by Delphini, Burchardi, Infessura, and Seracinus, the plague was raging at Rome in 1493 and 1494, the physicians observed in that city, no other malady but syphilis, or to conform ourselves to the language of the time, the *Morbus Gallicus*.

Nicholas Leonicensis, who holds so noble a station in the annals of medicine, for his courage in opposing the hydra of the superannuated theories of the Arabs, and whom Haller has, with propriety, placed at the head of medical reformers, assures us, that the popular name of *French disease* was given to syphilis, because the physicians had not appropriated to it a more proper one, and because it had been brought by the French, or at least had appeared during their stay in Italy. It is also certain, he adds, that the year of its appearance was rendered memorable by the general inundation of the country. Now, Infessura and Alexander Benedetti inform us, that this occurrence took place in October, 1493.*

Peter Pinctor, native of Valencia, and physician to the Pope Alexander VI. says, that from 1494 to 1499 a dreadful disease, called in Italy *French disease*, but differently at Valencia and other places, produced vast desolation in Italy.† Marcellus Cumanus, physician in the Venetian army, whose

* Liber de epidemiâ, quam Itali Morbum Gallicum, Galli vero Neapolitanum vocant, in 4to, Venetiis, 1497

† De morbo fædo et occulto, his temporibus affligente in 4to. Romæ, 1500. This work, which from its prolixity and barbarous style is excessively tedious, was quoted for the first time by Cotugno, in his work on the small-pox. Sanchez has made great use of it, in combating the American origin of syphilis. With patience, it is at last discovered, that the disease really originated in 1485 from the influence of the planets, but that it only assumed the true pestilential characters in 1494. Pinctor, from his astrological notions, is not at all surprised, that the disease should not have taken birth, properly speaking, either in Spain, France or Italy, as the conjunction of the planets to which he attributes it, extends all over the earth.

history is to us unknown, but whom we learn to have been at the siege of Novarra, gives a striking description of the syphilis he observed among his countrymen, in marginal notes added by him to Argelata's surgery. These notes were, a long time afterwards, collected by Rumler, first published by Welsch, and brought into general notice by Astruc.*

J. Cataneo testifies, that the *French disease* appeared in Italy in 1494, during the passage of the troops of Charles VIII.

Lastly, Nicholas Massa, a celebrated practitioner of Venice, asserts that the disease broke out in Italy—that it had not, as yet, received a proper name, but had been called by the people *French disease*—and that he himself had made use of this name, not with the idea of its correctness, but for the purpose of being better understood, this appellation having been almost generally adopted.

It naturally follows, from what we have said, that syphilis was propagated to a prodigious extent during and after the expedition of Charles VIII. From the same facts we must equally conclude, that it already existed at Rome and in Italy in 1494, according to Massa, Cataneo, Pincto, Burchardi and Capreoli: in 1493 at Rome and in Upper Italy, agreeably to Leonicensi, Fulgosi, Sabellico, Infessura and Delphinii; finally, according to Fulgosi, in Upper Italy in the year 1492. A Spanish physician of the name of Gaspard de los Reies Franco, has collected several passages from these authors, by which he has shown that the disease was known in Italy previous to the year 1493.† But supposing the testimony of Fulgosi too uncertain to be depended on, let us not admit that of Pincto, when he says the plague was raging at Rome in March 1493, and more especially when he mentions its existence in 1483; we cannot at least call in question the assertion of Infessura, who fixes on the month of June of the year 1493, as the epoch at which syphilis manifest-

* In his *Sylog. Curat. et Observat. Medicinalium*; in 4to. Aug. Vindel. 1668 T. 1. p. 53.

† *Elysium jucundarum questionum campus*. in fol. Brux. 1661, quæst. 59.

ed itself at Rome. We will therefore, for the moment, consider this as the date of the first appearance of the disease in Europe.

Next we shall review the principal circumstances attending the discovery of the New World, since as on these rest, in a great measure, the following discussion.

Christopher Columbus, animated with the sublimest idea ever framed by the human mind, that of the existence of another world, and of Providence having chosen him for its discoverer, obtained from the king of Spain, after meeting with refusals at Genoa, Portugal and England, three small vessels, and one hundred and twenty men, and the titles of Admiral and Viceroy of all the countries to be discovered. With these he embarked at Palos on the 3d or 4th of August 1492, saw St. Domingo the 6th of November, and left there for Europe on the 4th of January following. Being thrown by a violent storm on the Azores, he there landed on the 16th of February, and after a stay of a few days, continued his voyage and arrived at Val Parayso, near Lisbon, on the 4th of March, 1493, and met from John II. king of Portugal, a flattering reception. He remained nine days with this prince—again set sail, and reached Seville on the 15th of the same month. There he left his crews, and accompanied with six Indians who had followed him, arrived by land at Barcelona, to be presented to the king of Spain, who then held his court in that city. About the period of his arrival at court, the middle of April, one of his vessels, which had been separated from him during the tempest, reached Galicia.

We have before proved, from chronological researches the most accurate, laying aside all authorities of a doubtful nature, that syphilis was raging at Rome and in Italy about the month of June, 1493. This incontestable fact, by itself, excludes the possibility of its having been brought there by the army of Gonzalvo, as this general arrived in Calabria as late as the month of May, 1495. A part, then, of the romance of Astruc and Girtanner, is, by itself, overthrown. But as from the 4th of March, 1493, the period of the first

landing of Columbus, to the month of June of the same year, there is an interval of three months, during which the prejudiced might suppose, though with little probable appearance of truth, that the disease had been propagated from Spain to Italy—let us examine whether Europe is indebted to America for this pest, and whether syphilis really existed in the West Indies, and on the continent of the New World, at the time of the coming there of the Spaniards. By tracing in successive order the authorities adduced by Girtanner in support of his opinion, weighing their respective titles, and marking with attention the degree of credit which they deserve, we shall behold the already tottering edifice of the American origin of syphilis, give way even to its very foundations.

The first author quoted by Girtanner is Oviedo, whom he represents as having often associated with Columbus and his companions. Gonzalvo Ferdinand Oviedo, was born in the Asturias in 1478. He was sent to the court of Castille by his father in 1490, there placed at first in the house of the Count De Villa Fermosa, and afterwards as page near the person of the infant Don Juan. When Columbus returned from his first voyage, he was at Barcelona—and at Burgos, when the admiral, for the second time, arrived from America. After the death of Don Juan, which happened in 1497, he entered the service of the king of Naples, and soon after became attached to that of the queen. In 1513 he was sent to America, with the title of intendant of the gold mines of Daria—came back to Europe in 1515—soon revisited St. Domingo, and after remaining there ten years, returned to Spain, when he published his abridged History of the West Indies. About ten years subsequently, appeared his general history of America, of which we only possess the nineteen first books. He was, the same year, appointed Intendant of St. Domingo, from whence he was recalled by the Court in 1545, and in 1548 obtained the situation of historiographer to the king. From this sketch of the life of Oviedo, it will be readily perceived, that to point out a witness who could have better observed the various

events connected with the discovery of America, would be difficult, as he was in Spain when Columbus returned, went himself several times to the West Indies, and there filled the most eminent offices. He does not hesitate to ascribe the disease to the Americans, and to affirm that the Spaniards contracted it during their intercourse with the women of that nation. He adds that the disease must necessarily be endemic in the country, where the then supposed sovereign remedy guaiacum is native, as Divine Providence, who sends maladies to men as a punishment for their crimes, has always placed the remedy near to them. He was the first to advance that the disease had been brought to Italy by the troops of Gonzalvo of Cordova. He expresses his surprise that it should be called *French*, or *Neapolitan* disease, for he says it would be more properly denominated the *American disease*. In a word, to him may be ascribed the history of the propagation of syphilis, which has since been considered as a fact the most incontestable.

We must first observe, that Oviedo did not, as advanced by Girtanner, frequently associate with Columbus. No part of the writings of the Spanish historian can authorise this belief. Moreover, it is far from probable, that a man of the age and rank of the admiral, could have had much intercourse with a page of fifteen years. Besides, Oviedo only makes mention of the return of Columbus, in his abridged history. Now, he himself confesses to have written this work from memory, having left all his papers at St. Domingo. In his second work, composed from notes collected in America, he notices only the second voyage of Columbus. Naturally more confidence is to be placed in his great work than in one, traced from a memory most probably enfeebled by age—and it is to be borne in mind, that Oviedo, though citing many authorities for events of little importance, adduces none in support of the fact he here advances.

On the other hand, had this writer even established that syphilis really appeared in Spain at the time of the return of Columbus, we should still be justified in rejecting his testimony, for his works are filled with too many inaccura-

cies, infidelities and contradictions, to merit confidence. The facts he alleges are always doubtful, and their dates very erroneous: indeed by his cotemporaries, he was not held in much esteem. Ferdinando Columbus, the son of the admiral, accuses him of regarding as facts, the mere fancies of his imagination, and of forging, when necessary, authorities to support what he advances.

A. Herrera, an historian who, for his impartiality, his noble style and his attachment to truth, merits the greatest degree of consideration, reproaches him with the mean flatteries and gross falsehoods that abound in his works, which the following will serve to illustrate.* Oviedo maintains, that the Antilles are the Hesperides of the ancients, which had received the last name from Hesperus, king of Spain, 1500 years anterior to the Christian era, and therefore appertain, by all rights, human as well as divine, to the crown of Spain. Finally, Bartholomew de la Casa, who lived in America, and may be considered as an author no less respectable than Herrera, calls Oviedo's history false and detestable, and condemns Sepulvéda for having looked on him as unexceptionable authority. Oviedo constantly made a bad use of his power in the West Indies, for the purpose of oppressing the Indians. Dreading on his return to Europe, a merited punishment, he endeavoured, by representing this unfortunate people as wicked, dissolute, addicted to the most horrid vices, to luxury, to the worship of the devil, to atheism, and who had, for all these crimes, been doomed by Providence to be exterminated by the Spaniards, to justify himself before Charles V., and the fable he invented on syphilis served admirably his views.

Such is the man who is cited with confidence, and whose authority is considered of the greatest weight, by the defenders of the American origin of syphilis. What are we to think of the reports of an historian, despised by his cotemporaries, and by them convicted of so many errors, even had he stated in his great history, that the disease had

* *Historia general de los hechos de los Castellanos en las islas y tierra firme del mar Oceano* in fol. Madrid, 1601, Dec. v. l. x. c. 16.

appeared at the time of the first return of Columbus? He does not, however, bring it till the period of the second return, that is, 1496—which is too late, for we have already traced its existence in Italy in June, 1493.

The next writer cited by Girtanner is Rodriguo Diaz de Isla, whom he represents as being at Barcelona at the time of the arrival of Columbus, and having, soon after, written a treatise on syphilis. This author informs us, that not long after the return of the admiral, the city was infected with a horrid pestilential disease, and such was the fright it inspired, that to obtain its cessation, public prayers were ordered, together with fastings and other religious ceremonies.

Of the life of Diaz de Isla, we are totally ignorant, and his work originally written in Spanish,* is only known from a Latin translation, inserted in the collection of Welsch. We only learn that he practiced medicine at Seville. Whether he was at Barcelona, and frequently associated with Columbus, is very doubtful. He would not, probably, had it been true, have omitted to notice it. But there is no evidence in his writings to this effect.

According to Welsch, his work was only written in 1555, sixty-two years after the first return of Columbus, from which we are warranted to conclude he could not have been an ocular witness. Besides, when Columbus left Seville for Barcelona, his suite was composed of a few Spaniards and six Americans. How can we then for a moment suppose, that he, or those who accompanied him, could, in so short a time, have propagated so terrible a disease as that mentioned by Diaz? In fact, this plague appears to have existed only in his imagination, as no mention of it is made by Oviedo, who then resided at Barcelona. But if the silence of a man of so little weight should be objected to, that of Peter Martyr, then thirty-eight years old, and director of the college of the young nobility, instituted by the queen Isabella, cannot be. His letters,† in which he speaks of the arrival of Columbus, the wonderful things he brought,

* *Trotado contra las bubas*. In the *Sylog.* of Welsch, p. 31.

† *Opus Epistolarum* Alcalá, 1530.

the Indians who accompanied him, and the sensation created by so many novelties, do not notice the pretended plague of Barcelona. Neither does he say a syllable about it in his history of America, the most ancient work we possess on the discovery of the New World.*

The third authority cited by Girtanner, is Ferdinando Columbus, the son of the admiral, who wrote the life of his father from notes found among his papers.† He mentions, that when Columbus returned to St. Domingo, for the third time, in 1498, he found the colony had revolted, and was reduced to 160 men, all affected with syphilis.

It cannot, to us, be a matter of surprise, that the disease was found at St. Domingo in 1498, nor can we for a moment think it necessary in the explanation of this fact, to admit that it was endemic in that island. It is much more reasonable to suppose, that syphilis, known in Europe for the last five years, was brought thither at the time of the second voyage of Columbus. In no other part of the work, do we find any mention of the syphilis of America, or the plague of Barcelona: nor do we discover any thing on the subject, in the detail of the expedition of Columbus, written by Madrigano, or in the works of Vespuccius, Benzoni, Nigro and Anthony Gallo.

Another passage is quoted from the life of Columbus, alluding to a disease called *Caracaracol*, which, like the itch, produces many scabs on the skin, and is considered as identical with syphilis. This passage is to be found in a memoir written by Roman Pané, an ignorant Jesuit, whom the admiral had left in his first voyage at St. Domingo, for the purpose of learning the worship of the inhabitants, and endeavouring to convert them to Christianity. It proves nothing by itself, for syphilis is not the only disease that produces pustules and scabs on the surface of the body. Besides, it is given in connexion with a ridiculous story, that

* De orbe novo decades, in 4to. Alcala, 1500.

† La Historia del Almirante don Christoval Colon. in Bareia, historiæ, C. 73. C. 7. p. 63.

Pané had heard from the Indians themselves, on which he did not place much reliance, since he gives it the appellation of fable. To this I must add, that syphilis was never called *caracaracol* in the language of the Americans—but from these, received the appellations of *Huicavatl*, *Υχα*, and some others, as mentioned by Diaz de Isla.

Peter Martyr's testimony, according to Girtanner, is in favour of the American origin of syphilis. But besides that he was never in America, as mentioned by the German writer, the only passage which offers a plausible argument, averring that there existed in America a cutaneous disease produced by venereal excess, is not to be found in any of the original editions of the work, and as it only exists in the translation, it is manifestly added by the translator.*

According to Francisco Lopez de Gomara, a Spanish historian, the savages of St. Domingo were all affected with a disease, which was communicated by them to the Spaniards.† We are little acquainted with the life of Gomara. We only know that he was born at Seville, and became the chaplain of Ferdinand Cortez, whom he followed in his expedition to Mexico. His veracity has always been doubted, and he is constantly in contradiction with himself or with chronology. Oviedo is his only guide, and it appears evident, that on the subject of syphilis, he merely copied this last named author. He, in fact, makes use of the same arguments, and thinks that the existence of guaiacum in America is a strong proof of the origin of syphilis on that great continent.

Herrera speaks of the disease, as one very widely spread among the inhabitants of St. Domingo, and from whom it was received by the Spaniards. But it is not true, as asserted by Girtanner, that he had resided in America, as he himself proves the contrary in the preface, in which he replies to those who had objected to his writing the history of a country he had never visited. Besides, having written

* See p. 117, of the French Translation of Book de Orbe Novo.

† *Historia General de les Indias*, in fol. Caragoca, 1555. C. 12. p. 12. C. 19. p. 17. C. 90.

only in 1600, his testimony loses all the weight which has been attached to it. He borrows incessantly from Oviedo and Gomara, and we must believe, that, on the subject of syphilis, he has more particularly copied them, since at the time he wrote, it was generally thought on the testimony of Oviedo, notwithstanding his known inaccuracies, that the disease had really taken birth in America.

The opinion of the celebrated author of the history of Italy, Francis Guicciardini, has appeared of the greatest importance to the defenders of the American origin, on account of his scrupulous veracity, and because of his having been charged with a mission to the court of Spain. He, however, cites no authorities in its support. Can his testimony, allowing his merited reputation, balance that of Fulgosi and Sabellico, in deciding on a fact which happened in 1492-93, when it is recollected he was born in 1482, and was consequently but ten or eleven years old, whilst the two others were already of mature age?

I shall bring together all the authorities cited by Girtanner to prove, that the venereal disease was endemic on the continent of America, especially in Peru and the Floridas. These are Lopez de Gomera, Andrew de Leon, Augustin de Zaratte and Jerome Benzoni. All these authors make mention of a malady which was very widely spread in Peru, had received the name of *Berrugas*, and infected the greater part of the troops of Pizarro. The disease consisted in large red warts, of the size of a nut, attacking the nose, eyes, forehead, cheeks, and the surface of the body. Benzoni, who was himself affected with it, says it caused no pain. Gomara, although he considers this malady as a new one, asserts it to be syphilis. This is a most extraordinary contradiction: for if the disease were really new, how could Gomara say it was syphilis, which was already known thirty-nine years before, (Peru was discovered as late as 1531), and with which, supposing the disease to have really originated in America, they had already been affected at St. Domingo, as the most part of those who followed Pizarro, had before accompanied Columbus in his voyage.

This, among other gross contradictions of Gomara, should have served to open the eyes of the defenders of the American origin, had they not been blinded by the spirit of party. The same may be said of the Floridas, which were discovered by Cabot, seen afterwards in 1512, by Ponce de Leon, by Cartier in 1535, by Ribaut in 1562, and by Landonnier in 1564. What can the existence of syphilis in these provinces of Peru prove, since the main object is to ascertain whether this disease was really brought from St. Domingo to Europe in 1493?

With the other writers cited by Girtanner, may be mentioned Gabriel Fallopius, John Baptist Montano and Anthony Musa Brassavolo. These three authors wrote at too late a period to merit our attention, and have merely copied what was found in the works of their predecessors. It will, however, be necessary to pay some regard to Fallopius, to whose authority some importance has been attached. According to this physician, the returning vessels of Columbus were filled with persons affected with syphilis. He afterwards repeats the history of the propagation of the disease by the troops of Gonzalvo, and even adds to this, details extravagant and little honourable to the Spaniards, whom he accuses of having mixed lime with the flour, and of sending infected prostitutes among the French, for the purpose of communicating to them this horrid disease. Fallopius is, however, replete with historical inaccuracies. He gives four vessels to Columbus, and says he remained two years absent, whereas, the admiral left Spain with only three vessels, and returned after a voyage of seven months and eleven days. He says, likewise, that Naples was besieged by the French, who on the contrary, entered and left that city without meeting with the least opposition. Finally, we can judge from what precedes, whether the admiral could have brought back to Europe a whole crew affected with this disease.

Such is the abridged history of the American origin of syphilis, and of the very strong objections that have been advanced against it. Evidently this opinion was first pro-

mulgated among the Germans in 1518 and 19, but at that time its accuracy was held in doubt, for Peter Manard, professor at Ferrare, confesses in 1525, that he could not decide whether the disease came from the Antilles, or whether, according to the opinion of some physicians and historians, it was generated at the time of the expedition of Charles VIII, by the illicit commerce of a leprous man with a healthy woman—so as to form or constitute a degenerated leprosy. In that same year Oviedo wrote his history, the arrogant tone of which produced more effect, than had before been done by the writings of Schmauss and Van Hutten. Nevertheless, his opinion was not generally received. His countryman Francisco Delgado, who wrote on guaiacum in 1529, endeavoured to find the origin of the disease in any place but America, where neither Aloysius Lobera, Andrew Vesalius, nor Jerome Cardan, suspected it to have taken birth. A. Cesalpinus entertained doubts as to its transatlantic origin, and even in 1546, Jerome Fracas-tor, an intimate friend of Oviedo, could not conceive how a vessel from America was capable, in the course of one year, of spreading the disease all over Europe.

From the year 1518 to 1546, this opinion was therefore supported only by the faith of Schmauss and Van Hutten, the doubts of Menard, the unproved and uncertain accounts of Oviedo, and partly by the indecisive authority of Guicciardini. Fallopius contributed greatly in 1555 to its extension, by repeating the very words of Oviedo; and ultimately, by Astruc it was rendered a medical dogma.

The principal arguments of Schmauss, Hutten and Oviedo, are founded on the fact, that guaiacum is indigenous to America. But this article, according to Hutten, was only brought to Spain in 1508. How can we, therefore, credit, that the Indians, who at first received the Spaniards so kindly, could have waited, till by their atrocious conduct these latter had become justly odious in their eyes, to point out to them the remedy for a disease caught from themselves? Is it not more reasonable to suppose, that the utility of guaiacum in syphilis, is a recent discovery of the

Americans, who already knowing the virtues of this vegetable in several complaints, used it also with advantage to combat a disease brought to them by the Spaniards? Besides, were the arguments of Schmauss and Oviedo admissible, with as much propriety might we say, that syphilis had originated in China, from whence we derive the China root—or in Carniola and Frioul, where are to be found the principal mines of mercury, with which the disease is managed much more efficaciously than with guaiacum, or the China root? Lastly, however contagious we may believe the disease to be, can we, supposing it to have been brought from America, explain the rapidity of its propagation? As early as the middle of the year 1494, it had spread over Italy, France, the cities of Halle, of Anclam and of Brunswick, the countries of Brandebourg and Mecklenburg! The severe edict of the parliament of Paris, dated 6th of March, 1496, proves that it had already been raging two years in that capital. The one published on the 22d September, 1497, by James IV. gives us similar proof with respect to the city of Edinburgh. Conrad Schellig, who it appears wrote in 1494, says, that syphilis “*jam apparet in diversis regionibus.*” Such were its ravages in Hungary during the year 1496, that the king Vladislas, according to the historian Isthvansius, being terrified, fled from Buda, leaving the reins of government in the hands of his chancellor Bacoczy. Sebastian Brandt, whose poems were printed in 1498, also attests to the rapid diffusion of the disease.

It has therefore been proved, beyond a possibility of doubt, that the venereal disease was not derived from America. Indeed, would it not appear surprising, that a disorder arising from an immoderate venereal indulgence could have first developed itself among a people scarcely beyond the infancy of civilization, and who had not carried the depravity of their morals to as great a height as the Europeans, though Paaw has not hesitated to repeat the infamous calumnies of Oviedo, and to assert that the human species had entirely degenerated in America. Vices that are the consequences of luxury cannot be attributed to

man in a state of nature. Girtanner insists strongly on the libidinous disposition which the historians of America have attributed to the women of that country. Oviedo merely says that they yielded easily to the embraces of the Europeans, but were very reserved with the inhabitants of the country. La Casa and Herrera avouch the fact, and Vespucius adds, that the Indian women, having an irresistible propensity to the pleasures of love, and not finding the men sufficiently ardent to satisfy their immoderate desires, had recourse to stimulating drinks and to mechanical irritation. *Faciunt intumescere maritorum inguina in tantam crassitudinem ut deformia videantur et turpia; et hoc quodam earum artificio et mordicatione quorundam animalium venenosorum, et hujus rei causâ multi amittunt inguina, quæ illis ob defectum curæ flaccescunt, et multi eorum restant eunuchi.* This story, which appears little probable, countenances too much the opinion of Girtanner, to have allowed him to omit it. He even thinks that it will serve to explain the first development of the disease in America, and believes that the virus of the insect introduced into the vagina of the female by the man, produces there ulcers which afterwards are communicated to those with whom she has intercourse. It must, however, be confessed, that this theory is considered by Girtanner as a mere conjecture. It is, indeed, about as ridiculous as that of Heydentryk Overcamp, who thought that the disease had originated in America from the intercourse of the Indians with brutes. This idea had already been advanced by Vanhelmont, and afterwards supported by Laurent Roberg and John Linder, physicians of Upsal. It may be placed on the same footing with that of Martin Lister, who, applying a wrong sense to a passage of Oviedo, believed the venereal disease to have arisen from the use of the flesh of a species of lizard called by naturalists *Iguana delicatissima*. It is not more absurd than that of A. Hauptman, Christian Lange, David Overcromby, Nicholas Hartsocker, A. Calmet, A. Deidier, and Herman—Paul Iuch, who supposed, conformably to the doctrine of Athanasius Kircher,

that the syphilitic symptoms depend on worms, that insinuate themselves in the soft parts and gnaw them. All these suggestions are not less preposterous than disgusting; though none is more in opposition to chronology, history, and good sense, than that advanced by Joseph Xavier Rechman, according to whom, syphilis originated in Egypt, from whence the Jews, having contracted it during their captivity, brought it to Palestine, where it was extended to the Christians during the time of the Crusades.

(To be continued.)

ART. III. *On the Generation of Animal Heat. Read before the Philadelphia Academy of Medicine.* By RICHARD HARLAN, M. D. &c.

“Opinionum commenta, delet dies Naturæ judicia confirmat.” CICERO.

EARLY convinced of the inefficacy of those theories founded upon the doctrines of *chemical life*, which have a natural tendency to debase man from his dignified and compound condition of soul and body, to a mere complex chemical machine, I was led to examine with care the laws of vitality, and the *phenomena* resulting therefrom—not the least interesting of which is the *generation of animal heat*, or that function by means of which the body during health, maintains a similar and equable temperature throughout all parts of the globe.

I may here be permitted confidently to state, that I have been governed in my investigations solely by the desire of detecting truth. “Friend to no sect I seek no private road.”

Clothed in the seductive garb of high toned eloquence, or subtle disquisition, error appears to have been inseparably connected, with those dogmas which allege that the multiplicity of changes, synthetical and analytical, occurring in our elementary particles, are produced by means of chemical or mechanical agency! Numerous examples might be adduced in illustration of this, though I shall rest satisfied with the following well attested fact.

While one physiologist estimated the force of the heart, as equal to one hundred and eighty pounds, another reduced it to eight ounces, and both these conclusions are deduced from reasonings presented in all the imposing forms of the exact sciences! It has well been said, that our ignorance may be concealed, but cannot be removed, by the vain parade of a *science foreign to medicine*.

In the following pages I shall have frequent occasion to make use of the terms *organization*, *vital properties*, *functions*, and *life*. My time and the limited space allowed in the pages of a journal will not permit me to enter into the discussion of that already hacknied subject, *vitality*. But it is necessary here briefly to state, that I consider the above expressions intimately related to each other—*vital property* is the *acting power*—*organization*, the *instrument acted upon*—*function*, the *mode of action*, and the *phenomena* of life the *result*.

To the pride of man it is not a little mortifying, boasting as a mark of pre-eminence the *progressive knowledge* of his species, that we are obliged to retrace our steps, through the difficult and arduous paths of elementary science, back to the days of the immortal Hippocrates, a period upwards of two thousand years, for a *theory* of animal heat—which, after the successive labours of the innumerable hosts of chemists who have flourished since, until the appearance of Hunter and Bichat, (whose ideas on this subject we shall see differ very little from the father of medicine,) possesses more of the semblance of truth, and is more in consonance with the immutable laws of life, than is offered by any author with whom I am acquainted.

The generation of animal heat, was thought by Hippocrates, to depend upon the action of a "*vital principle on organized matter*," and hence, that the function is not confined to any particular organ or part of the body.

About the period in which Boerhaave flourished, the science of physiology was subjected to a revolution equalled only by the ebullitions produced by Des Cartes in natural philosophy. The vigorous mind of Boerhaave was not ex-

empt from the infatuation which seized upon the writers of the age, who resolved all the vital phenomena into attraction and repulsion. To the attrition of the fluids upon the solids of the body, or to the friction of the particles of blood against the sides of the vessels, or to fermentation and putrefaction, was successively ascribed the temperature of animal bodies.

To the first, it was objected, that no perceptible heat has ever been excited by the attrition or agitation of water or oil, quicksilver or other fluids, unless they have undergone at the same time some chemical change, as in agitating milk or wine till they become sour.

The two latter causes, or fermentation and putrefaction, would not be maintained at the present day by the least informed chemist.

It was thus, that hypothesis succeeded to hypothesis, without any thing satisfactory having been offered, till Dr. Black gave to the world his celebrated theory of latent heat. Comparatively ignorant of physiology, he was led to draw false deductions from the most clear premises, though his main principle, that latent heat is converted into free caloric, will be found to stand. Nevertheless, he erroneously confined the calorific function to the pulmonary organs, alleging, that in consequence of a reciprocal action of the blood in the lungs, and of the atmosphere, a portion of the latent heat of the latter was absorbed.

Nearly allied to the preceding is Dr. Crawford's theory, the essence of which is, that the capacity of arterial, for heat, is greater than that of venous blood—that there is no difference of temperature between the two ventricles of the heart—and in fact, that the heat of all parts is nearly the same.

This apparently sound, and certainly beautiful theory, gained many advocates, and was admirably calculated to enlist such as are inattentive to the peculiar laws of life, or warm in the pursuit of a favourite science. But I hold it to be entirely untenable, as at variance with the fundamental laws of the animated being. "Whatever healthy

mutations, whether of the solids or fluids, take place in that admirable workmanship, the human body, are the result, not of *chemical* but *vital* agency—an agency independent of organization.”

Next. I proceed to state my objections to this pulmonic theory, which has become so fixed and rivetted on the minds of many of the older, or electro-chemical physiologists, as scarcely to be shaken.

The lungs I shall at present consider as grand emunctories, whose principal function consists, in eliminating an excrementitious matter out of the mass of blood. I readily admit, that the oxygen respired, partly disappears, and that carburetted hydrogen is formed: it also seems probable, that the air respired may assume a more condensed form, or enter into combinations, producing new materials, possessing less capacity for heat. But it has never been proved, that the capacity of arterial blood for heat, is greater than venous blood, though it is reasonable to suppose, that the materials to be discharged from the lungs must assume a more fluid form, and thus *absorb* heat. The sensible temperature of the blood in both ventricles is granted to be the same. Nevertheless nearly all the phenomena of health and disease militate against the chemical views of the origin of animal heat—some of the more striking of which I shall notice.

We frequently observe a local increase of temperature, as in the act of blushing—in topical inflammation—in chronic hepatitis, which causes a burning of the cheeks, of the palms of the hands, soles of the feet, &c. which cannot be explained by any of the theories of the oxydation of the blood.

Heat, moreover, continues to be evolved from the body some time after *apparent death* from acute diseases. I have dissected bodies warm twelve hours after death, while others which died of a lingering disease, though otherwise under similar circumstances, have cooled in two or three hours.

The pulse in certain stages of fever is diminished in force

and velocity, the system nearly exhausted, and the lungs in particular, scarcely able to perform their office—and still, we find the temperature of the body morbidly increased.

In phthisis, asthma, apoplexy, &c. the same phenomena present themselves.

A remarkable instance of the latter disease came under the observation of Mr. Hunter: “a gentleman was seized with an apoplectic fit, and while he lay insensible in bed, covered with blankets, I found,” says he, “that his whole body would in one instant become extremely cold in every part, continuing so for some time, and as suddenly became extremely hot; while this was going on, alternately, there was no sensible alteration in his pulse for several hours.*

That the heat of the body does not depend upon respiration, or the state of the circulation, has indeed been noticed by many writers. It is remarked by Dr. Rush,† that “during the fever of 1794, he found, that the burning heat of the skin, called by the ancients, ‘*calor mordens*,’ and from which this disease, in some countries, had derived the name *causus*, was more common this year, than the last; it was sometimes local, sometimes general; it had no connexion with the state of the pulse, or circulation, for it was most intense at a time when the patient had no pulse.”

We are told by Dr. Chisholm that he “found the skin to be warm, whilst the pulse was at fifty-two degrees; and that it was sometimes disagreeably cold, when the pulse was as quick as in ordinary fever.”

Exactly the same was observed by Senac in intermittents, “I have often seen,” says he, “the lower extremities extremely cold and the upper ones parched with heat.”‡

By some experiments of Mr. Hunter, it is proved, that the egg during incubation has the power of generating heat, in which case, pulmonary influence is out of the question.

“Having taken,” he observes, “some eggs from under a hen, when the chick was about three-fourths formed, I broke a hole in the shell, and introducing the ball of the

* *Animal Econ.* p. 91.

† *Med. Inq.* p. 212.

‡ *Senac*, p. 25.

thermometer, found that the quicksilver rose to ninety-nine and a half degrees. In some that were addled, I found the heat not so high by two degrees, so that the life in the sound egg, assisted in some degree, to support its own heat.”*

He entertains the opinion that this power of generating heat is peculiar to an animal while alive: it is a power only of opposition and resistance, since it is not found to exert itself spontaneously, but must always be excited by the energy of some frigorific agent or disease.

“It does not depend upon the motion of the blood, as some have supposed; because it likewise belongs to animals who have no *circulation*, and the nose of a dog, which is always nearly of the same heat, in all temperatures of the air, is well supplied with blood. Neither can it be said to depend upon the nervous system, for it is found in animals that have no brain or nerves; it is then most probable, that it arises from some other principle, a principle so connected with life, that it can, and does exist, and act independently of circulation, sensation, and volition, and it is that power, which preserves and regulates the internal machine. This power of generating heat is in the highest perfection, when the body is in health; and in many deviations from that state, we find that its actions are extremely irregular.”†

Much has been said by the advocates of the pulmonic theory, concerning the temperature of the blood in the left side of the heart, being higher than that in the right. Experiments to ascertain this point, differ very much in result. It is asserted by Drs. Crawford and Davy that there is no difference of temperature, while some on the other hand, have maintained that the temperature was highest in the right ventricle.

If any difference exists between the arterial and venous blood, in the cavities of the heart, as respects their specific caloric, excepting what arises from the difference of specific

* Animal Economy, p. 103.

† Idem.

gravity,* it can only be accounted for by the superior power of the left ventricle, its parietes being composed of much more numerous muscular fibres, its actions much stronger, and the formation of animal heat in the part might be greater.

The temperature of parts differs according to the degree of vital action present. Mr. Davy found in his experiments, that in the stomach of an ox, the pyloric compartment was of a higher temperature than the left ventricle itself. Thus when the latter immediately after death was 103, the former full of food was 104, 5, from the stimulus of its contents and increase of vital action.

This enables us to explain the fact, that the temperature of young animals in whom all the vital actions are most energetic, is higher than adult animals, and much greater than in old persons—which was remarked by Hippocrates, "*qui crescunt, plurimum habent calidi innati, senibus autem paucus calor.*"

In confirmation of this remark, we have the experiments of Davy,† who found in one instance, the heat under the axilla of a child just born, 98 5, after twelve hours, 99 degrees, and after three days the same—during the whole of which time, it appeared in perfect health. On five other children of the same age, similar observations were made: in two instances of weak infants, the temperature one hour after birth did not exceed 96 degrees, which is two degrees below the standard heat of a man in perfect health—probably from the *general debility* of the subject of the experiment.

Of late, we have a theory, which attributes the production of animal temperature to the influence of the nervous system. The work of Dr. Wilson Phillip on this subject, has attracted no inconsiderable share of attention. He supposes it to be generated by the action of the nervous influ-

* The specific gravity of venous blood, without its fibrine, is 1050, that of arterial 1047.

† Eclectic Repertory, 1816.

ence on the blood, by which, the formation of secreted fluids is effected, and consequently that heat is a secretion.

An hypothesis like this, could only have been framed on limited views of the animal economy. We are very liable to error by drawing hasty conclusions from a few isolated facts, as they occur in the higher grade of animals, without availing ourselves of those convincing evidences which are to be collected, by observing the phenomena presented in similar functions, in a more extensive survey of animal existence. Thus in the theories of animal heat, we have confined ourselves generally to the phenomenon as it exists in the vertebral animals.

Had Dr. Phillip carried his observations on the animal economy a few steps further, he would have saved himself the necessity of retracting opinions which cannot be supported by a comprehensive view of the subject.

In relation to the lowest class of animals and vegetables, in which no nervous system has been discovered, Mr. Hunter found that the most imperfect are capable of an evolution of caloric. It seems to me that it is by no means established, that secretion is materially dependent on the nervous influence: on the contrary, many facts are opposed to the supposition.

Man is emphatically from the beginning, a secreted animal. In the fœtus in utero, assimilation probably is the only active function existing, at least, the whole glandular system, in the human fœtus, is quiescent: it is not till the blood has received the influence of the atmosphere, that we have urine, bile, saliva, &c.

The nervous influence being essential to the evolution of heat, how is it that the chick in ovo is capable of evolving it? in which, the heart may be seen performing its functions, when no appearance of brain, or spinal marrow, can be traced. If the sanguiferous and nervous systems were co-existent, the formation of the animal must begin at more than one point, which is contrary to the simplicity observed in the established laws of nature.

Were the nerves at all concerned in the formation of

animal heat in the human subject, the influence must reside in those of organic sensibility solely, since we not unfrequently witness a total loss of nervous energy, while the power of evolving heat remains.

We have a case detailed by Dr. Parry, in which there was an extinction of pulse in one arm, with a coldness, though complete power of motion existed in it, while the other arm was warm, and had a perfectly good pulse, without, however, the power of voluntary motion.*

I witnessed a case some years ago, in the Pennsylvania hospital, which tended to prove that neither respiration nor nervous energy has much connexion with the function under review. It was a patient with a fracture of the dorsal vertebræ, and consequent pressure of the spinal marrow. The inferior extremities were totally deprived of nervous influence: and yet, the portions of the body above the injury were of a natural temperature, while all the parts below the fracture indicated by the thermometer a morbidly increased temperature. It is as needless as it would be easy to multiply facts of this nature.

In our speculations on animal temperature, we must at present be content with the knowledge of facts. To refer it to some general principle, belongs probably to the destiny of future ages. I cannot forbear, however, to cherish the suspicion, that heat is caused by a distinct and peculiar action of living matter. Be it called a secretion, it is a peculiar one, though like the secretions in general, it is modified, increased or diminished by the operation of the passions and other states of mind.

Considered as a secretion, animal heat most probably enters the system with the chyle, in a latent form, and is set free by the action of the vessels upon their contents. What else produces those changes in the blood denominated secretions? Every one now seems convinced by direct experiments, that vessels so modify their contents, that the liquids and substances which the absorbents take up, become

* Parry on the Pulse, p. 139.

lymph, a fluid resembling blood, in their transit through the vessels, and that the chyle imbibed from the bowels is undergoing continual modifications, as it proceeds through its channels to the sanguiferous system.

There are secretions which not only are modified, but entirely wanting as we descend in the scale of beings. Birds, for example, have no salivary glands, and precisely the same occurs respecting the formation of animal heat, in that class denominated cold-blooded animals, as frogs, toads, lizards, snakes, &c. which "have no power of generating heat within themselves,"* notwithstanding they breathe with lungs.

What effect low diet or starvation has, in diminishing the power of forming animal heat, is generally known. Do we not often meet with persons, who have been accustomed to abstemious living and otherwise temperate habits, with hands continually as cold as a lizard?

Warm-blooded animals inhabiting the ocean, perhaps from this circumstance alone, require a greater power of economising food, as the very action of keeping up animal heat may require an additional supply of nourishment.

In searching Bichat for his authority, I was glad to find that I did not very essentially differ from so distinguished a physiologist. "*Le dégagement du calorique*," says this author,† "*est donc un phénomène exactement analogue à ceux dont le système capillaire général est le siège.*"

Again, "*Le calorique arrive donc au système capillaire combiné avec la matière des sécrétions, avec celle des exhalations et celle de la nutrition. Le sang est le fluide commun qui résulte des toutes ces combinaisons. Dans le système capillaire général, chaque partie se sépare, le calorique pour se répandre dans tout le corps et sortir ensuite au dehors; les fluides, des sécrétions pour sortir par les glandes; ceux des exhalations pour s'échapper par leur surfaces respectives; les nutritifs pour séjourner dans les organes.*"

Further, "*chaque système a son mode particulier de chaleur. Certainment il se sépare moins de calorique dans*

* Home's Comp. Anat.

† Anat. General, vol. ii. 523 and 524.

les cheveux, les ongles, l'épiderme, que dans tout autre système. Les organes blancs, comme les tendons, les aponeévroses, les ligamens, les cartilages, etc. en fournissent aussi moins probablement que les muscles. Examinez les pâtes des oiseaux ; ou il n'y a que ces parties blanches ; elles sont bien moins chaudes que le reste du corps."

He, however, admits that some latent heat may pass into the system through the medium of the lungs, and attempts to explain in this manner the connexion between the size of the lungs and the quantity of heat disengaged. But it is not unlikely we shall find on examination, that the degree of heat evolved, is in proportion also to the quantity of ingesta.

Not long ago I heard it argued by a distinguished chemist, that animal temperature is produced by the assimilating functions, on the principle, that fluids converted into solids give out heat. But it was objected, that it is an acknowledged fact, that the absorbents are as active in converting the solids of the body into fluids. Each of these theories are I think, to be discarded for the same reason, namely, that the ultimate particles of all bodies are similar, whether solid or fluid.

It may be added, that the temperature of a part is not increased proportionately to the assimilating function in energy, and conversely, as instanced particularly in the case of paralysis cited above.

I have thus endeavoured to show, chiefly by a train of negative reasoning, and I hope not unsatisfactorily, that the generation of animal heat, and the function of respiration, have very little if any connexion with each other, and in some animals, none at all. Believing ignorance to be preferable to error, no small point is surely gained, if former theories are proved to be fallacious, or at least inadequate to explain the phenomena of the case. It is not to be expected that this subject will be divested of obscurity, till we become much better acquainted with the laws of life, the importance of which, to physiology, and to the theory and practice of medicine, cannot be too highly estimated, though such inquiries have been by many condemned as purely speculative.

We do not hesitate to calculate the fall of an apple from the tree—the projection of a ball from the mouth of a cannon—or the revolution of the moon in her orbit, knowing as little of gravitation, attraction and projection, as we do of the cause of muscular motion, animal temperature, or any of the vital operations.

We are indebted for the ensuing interesting article, to our friend Dr. John Bell, to whom it was transmitted by a correspondent at Paris, who received it from the authors. It has never before appeared in print.

EDITOR.

ART. IV. *On two new Alkalies, Cinchonine and Kinine, discovered in the Peruvian Bark.* By Messrs. PELLETIER and CAVENTON.

MESSIEURS Pelletier and Caventon, convinced by their researches into the vegetable alkalies that a great number of vegetable substances which exert strong powers over the animal economy, owed their energy to a salifiable base, as the morphine in opium, the strychnine in the nux vomica, the veratrine in the veratrum, &c., began a course of experiments on the Peruvian Bark, with the view of ascertaining whether it did not contain a regular salifiable base, to which its virtues were attributable. At first, more especially occupying themselves with the pale bark, they soon observed that the crystallizing principle discovered by Dr. Garnil, of Lisbon, and which he had called *Cinchonine*, (seen also and described by Mr. Lauber under the name of the white matter or resin of the bark,) was a substance of this kind, only united to an oily matter. When prepared agreeably to the process of Dr. Garnil, it had not been dissolved in a weak acid largely diluted, then precipitated again by a more active salifiable base. Its property of saturating the acids once recognized by Messrs. Pelletier and Caventon, they set about establishing its chemical nature, in considering it

as a salifiable base. They determined its point of saturation, and the weight of its molecules, which they found to be 38,488. They combined this substance with different acids, and ascertained the form, proportion and properties of its salts, which all have a bitter, *sub-aromatic* flavour proper to the pale bark. The cinchonine itself has this peculiar taste, but much less marked, owing to its very slight degree of solubility. Some neutral salts of the cinchonine, such as the tartrate, oxalate and gallate, being also insoluble, have a corresponding weak taste, which is however developed by an excess of acid which augments their solubility.

Messrs. Pelletier and Caventon continuing the examination of the pale bark, determined also the nature of the other substances of which it is composed, and which joined to the cinchonine are as follows :

1. Cinchonine united to the kinic acid.
2. A green oily matter.
3. *Cinchonic* red.
4. Red soluble colouring matter, (tannin).
5. Yellow colouring matter.
6. Kinate of lime.
7. Gum.
8. Starch.
9. Ligneous matter.

If we enter into a consideration of these substances, the properties of which have been examined in detail and with care by the authors of the memoir, we shall see that the greater number, viz. the cinchonic red, the oily matter, the gum, the ligneous matter, and the starch, cannot possess any of the medicinal properties of the bark, of which they have not the taste : the greater number of them are even insoluble. The soluble colouring matter or the tannin cannot either be regarded as the active substance in the cinchona, for if the latter owed its virtues to any tanning matter, other barks very rich in this principle, ought also to have the properties of the cinchona in proportionate energy according to the quantity of tanning matter they contain, but which is contrary to medical experience. There remains the kinate of

lime, but this salt when pure has no bitter taste, and is besides insoluble in alcohol. The alcoholic tinctures of bark, therefore, do not contain it, and they are nevertheless febrifuge. Every thing then induces us to believe that the active principle of the bark is the cinchonine; consequently, it is to this substance and to its salts, that the attention of the physician and his therapeutical researches ought to be directed.

Passing on to the analysis of the yellow bark, Messrs. Pelletier and Caventon, in place of detecting the cinchonine, obtained a substance not crystallizable, and differing from the former in its physical and chemical properties. It is a salifiable base, but its capacity for acids is different from that of the cinchonine, and its salts are formed in proportions respectively different, distinguishable for the most part from those of the cinchonine by their form and appearance: their taste is also more bitter—in this respect having a nearer identity to that of the yellow bark. The above gentlemen, after establishing the difference which exists between the salifiable base of the pale and of the yellow bark, have thought proper to call that of the latter *kinine*.

Messrs. Vauquelin, Thenard and Beyeux, a committee of the Academy of Sciences, have admitted the difference established by the authors of the memoir, between the cinchonine* and the kinine, and have approved the name given to the latter substance, which seems to be to the yellow bark what the cinchonine is to the pale bark. Therapeutical experiments have also been made and even in greater number on the kinine.

The red bark has in its analysis presented a very extraordinary fact: it is the simultaneous presence of the *cinchonine* and of the *kinine*, and each in greater quantity than is afforded by the pale and yellow bark. The red bark then is very justly regarded the best.

* These gentlemen have suggested the addition of the letter *e* to the cinchonin, the name first given by Garnil, to the authors of the Memoir, so as to have the name cinchonine in conformity with the names of the other new vegetable alkalies, as morphine, strychnine, brucine, &c.

Messrs. Pelletier and Caventon have examined other varieties of the cinchona, but their experiments are not yet terminated.

Process employed by Mr. Pelletier for procuring the Cinchonine and Kinine.

Repeated alcoholic tinctures of the bark are first made and by evaporation the alcoholic extract is obtained. It is in this extract that the cinchonine or the kinine is found, which exists in the Peruvian bark. To obtain the alkaline substance of a suitable purity, the extract is boiled in a certain quantity of water, to which has been added a few drops of hydrochloric (muriatic) acid: the liquor after cooling is filtered,—then concentrated, and treated with an excess of magnesia,—boiling it for a few minutes, the liquor is again suffered to cool and then filtered. The precipitate received on the filter is composed of kinine, calcined magnesia, tannin, and cinchonic red. Wash the precipitate with cold water, dry it in a sand bath, then treat it with boiling alcohol, which dissolves the alkali and leaves the magnesia and the tannin united to the colouring matter. It remains but to evaporate the alcohol, to obtain the kinine of a superior purity.

The alkali of the cinchona thus prepared is sometimes impure by an admixture of oily matter—to separate which and purify it, we must dissolve it anew in an acid largely diluted with water, filter again the liquor, and treat it for the last time with magnesia and alcohol, as has been already mentioned.

Medical Use.

The *sulphat* of *kinine* has been administered by Mr. Double, a distinguished physician, to the extent of four grains daily, two in the morning, and two in the evening, which has sufficed to make the fever disappear: but to prevent a relapse its use was continued some days longer.

In other experiments made at the Hopital de la Charité, five grains have been given in simple quotidian fevers, and the same quantity continued for eight days: from the very first dose the recurrence of the paroxysms was prevented.

ART. V. *Cursory Remarks on Inverted Toe Nail. Read before the Academy of Medicine of Philadelphia.* By CHARLES MEIGS, M. D.

IT may, perhaps, be not improper to occupy a small portion of time, with some observations concerning a troublesome and obstinate disease, which, though apparently of small consequence, proves not unfrequently a source of much pain to the patient, and by baffling his attempts at a cure, of much vexation to the practitioner.

In books of surgery, a variety of plans have been proposed for the cure of what is called inverted toe nail. They are not only painful, but what is a more serious objection, are of very uncertain efficacy. I have lately treated a few cases with a favourable result, by a simple, easy, and speedy method.

In the morbid affection under consideration, an idea prevails among patients as well as physicians, that the nail is grown down into the flesh: but I am disposed to think that this is not correct, though I would not pronounce decidedly on this point, since my observations are limited.

It is probable that the disease is generally produced by the imprudent practice among some people, of cutting at the lateral edge of the nail, with a sharp-pointed penknife. In this process, if the skin is once abraded in that place, a sore is formed, which is perpetually irritated by the pressure of the nail from above: and the injury is rendered more severe by means of the sliding of the nail over the wounded part. The nail here acts as an extraneous substance, bruising and rubbing the abraded spot, till considerable portions of fungous granulations make their appearance, and once in this state, it requires only the action of walking to keep up the disease to an indefinite period. I have seen the great toe and a portion of the foot of a young gentleman involved in a very painful inflammation in this manner—and as the nail does not yield its position, it appears, by the swelling of the soft parts on one or both sides, to have grown down into the flesh—while in reality, if the fungous granulations, and

the tumefaction of the side of the toe were subdued, we should see the nail occupying its proper place, being no deeper at its edge than is natural.

It were useless to consume time by describing the several methods of treatment which are recommended. To be brief, I shall merely observe, that the extirpation of a portion of the nail, is always intensely painful, and sometimes dangerous.

The toe and part of the foot itself being highly inflamed, the usual means of reducing inflammation are to be directed. But when the inflammation is confined to the side of the toe under the edge of the nail, the following method may be at once adopted.

Let a small pledget of lint, just large enough to cover all the granulations, and of sufficient thickness to act as a compress, be neatly adjusted, over which a roller of linen three quarters of an inch wide, and eight or ten inches long, is to be applied, having one end previously spread with adhesive plaster. By this method we are enabled, with great ease, to make it act not only on the compress, which will destroy the granulations very rapidly, but by confining the toe and nail; to prevent even the *small* degree of sliding motion or friction formerly mentioned, thus doing away one principal cause of the disease.

By pursuing this treatment, the patient will generally recover, even while walking about—and the pain certainly is removed very quickly, for he can now wear a shoe, who before found a tight stocking inconvenient.

Commonly we find that the inverted edge, as it is called, is cut, and where it is done so as to leave it ragged, it may be smoothed afterwards, though the less it is disturbed, the better. In conclusion, I beg leave to observe, that the last stage of the cure is most difficult, because cicatrization can not take place easily, unless the part is at rest. Hence the patient will say, "it is almost well, but does not get any better." All that is required, under such circumstances, is a state of complete repose for a few days. It will be right to continue the roller for a short time after the part is healed.

I will make no other apology for this minuteness, except that he who has seen one half the nail partly dissected, and partly torn off, will be disposed to overlook the air of trifling displayed, by using so many words concerning so small a matter.*

ART. VI. *On the effects of Atmospheric Air, when applied to the parts of the body, which are not designed by nature to be in contact with it. Read before the Academy of Medicine of Philadelphia.* By HENRY BOND, M. D.

“Bind up my wounds.”

EVERY part of the body, which is accessible to atmospheric air, is covered by that peculiar and important membrane, called the cuticle, or epidermis. The only exception to this is in the teeth, which have a covering, in some respects, resembling the cuticle; and perhaps in the nails, which by some are considered but a process of this membrane, and certainly resemble it in their chemical composition. This membrane not only covers all the external parts of the body, but descends into, and lines every ca-

* The fifth volume of the Medico-Chirurgical Transactions contains a valuable paper by Mr. Wardrop, of London, on the diseases of the toes and fingers, from which we extract so much as particularly relates to the treatment of the affection above described.

EDITOR.

“The chief point to be attended to in the treatment of this disease is, not to cut away any of the nail, but to reduce the swelling of the soft parts. I have generally found that one or more applications of the lunar caustic have had the effect of entirely removing the swelling of those parts. The caustic destroys the painful and irritable ulcerated surface, whilst at the same time it promotes absorption of the thickened parts. In some cases it has been thought necessary first to alleviate the accompanying inflammatory symptoms by the application of poultices, and in some instances also, the alumen ustum has been found to answer better than the caustic; but in all cases, these means succeeded in curing the disease.

“It may here be remarked, that in order to prevent either the recurrence or the formation of a disease of this kind, care should be taken not to cut off the corner of the nails, particularly that of the great-toe, for when this is done, the shoe presses the adjacent soft parts against the sharp edge of the nail, and thus produces pain and inflammation.”

nal and cavity, which are continuous with the superficies. Its organization, if it have any, is very simple. It has no nerves, and consequently, no sensibility. It has never been proved to be vascular; and, upon examination, seems to have little better claim to be considered organized, than the varnish of cabinet wares, and perhaps, is intended to answer the same purpose. But, although neither nervous nor vascular, it is certainly pervious, and if it have any organization, it probably consists principally in the peculiar structure of those perforations, which perform the function of perspiration, and of cuticular absorption, if this be allowed still to be classed among the sound doctrines of physiology.

Various opinions have obtained among physiologists, with respect to the permeation of perspirable matter through this membrane; but they are so remotely connected with the subject of this paper, that I shall omit their consideration. None of these, however, claim for the cuticle a higher grade of organization, than I have just admitted to be probable.

The thickness and strength of this membrane vary on different parts of the body, according to the usual liability of the part to exposure to the action of chemical and mechanical agents; and they vary in the same part according to its habitual circumstances of exposure. Continued pressure will produce a thickness of the cuticle, wherever the subjacent parts are sufficiently firm to allow pressure to be continued. In this power of accommodating itself to circumstances, we see how admirably the cuticle is adapted to answer one of the uses assigned to it by physiologists, namely, to preserve the skin from wearing. Like charity, "it gains by what it loses."

By its insensibility, it is likewise admirably adapted to another use assigned to it, which is, to defend the true skin that it may not be exposed to pain from whatever it touches, especially the atmospheric air. Such is the sensibility of the true skin, that without the interposition of this defence, we should be literally

"feelingly alive all o'er,
"To smart and agonize at every pore."

The cuticle also possesses a very considerable power to resist the action of chemical agents. It is insoluble in water, and retains the moisture of the skin to such a degree, as to keep it soft and flexible.

Added to the other properties which I have mentioned, is its transparency, which, from our imperfect knowledge of the agency of light on animal bodies, seems more important in reference to the social relations of the individual, than to that of the animal economy.

One other circumstance, to which I have not yet adverted, shows the importance of this membrane, which is, that the cuticle, or something analogous to it, is found to invest, not only animal, but all vegetables universally, whenever they are exposed or liable to exposure to the air, and it may be distinctly demonstrated on the embryo very soon after the development of its parts. This universality of the cuticle would seem to indicate, that there is some such power or property in atmospheric air, that all living beings, whether animal or vegetable, require a defence against it.

That atmospheric air, admitted to parts of the body, which are not covered by cuticle, is productive of injury, is a most familiar fact, and our practice is directed accordingly, whenever we dress a blister, a burn, an ulcer, or, in short, whenever the continuity of the integuments is interrupted by an operation, an accidental wound, or by diseased action. Whatever may be the speculative opinions of men with respect to the existence of a principle in the air, which is particularly prejudicial to raw surfaces, I believe the practice of excluding air from parts of the body not defended by cuticle, is very prevalent. One important improvement in the modern practice of surgery is, that now, instead of keeping a wound open by balsams and other stimulant substances, in order that it may suppurate and incarn, its lips are made of such shape and dimensions as to come into apposition, and the dressings are performed in the very same way they would be, if the only object were to present nothing but cuticle to the action of the air.

Mr. John Bell, nevertheless, considered it indifferent, whether a raw surface were left exposed to the air or not. I will give the opinion of Mr. Bell on this point in his own words. "That the vulgar," says he, "should believe the first impression that strikes them, of air hurting a wound or sore, is by no means surprising; but it is not natural that men bred to philosophy, should allow so strange an assertion as this, without some kind of proof. That the air we breathe, and which we feel upon the surface, so bland and delightful, should have so opposite a relation to the internal parts—that it should be a stimulus more acrid and dangerous than the urine, is not to be believed on slight ground. I do affirm," says he, "that it remains to be proved, that this fluid, that seems so bland and pleasant to all our senses, and to the outward surface, is yet a horrible stimulus, when admitted, as a celebrated author expresses it, into the deep recesses of the body."

It must immediately appear to the least critical observer, how unphilosophical Mr. Bell is in this extract, in which he talks so superciliously of the bad philosophy of others. In the same manner he might infer, that because we can handle most of the salts, and apply them to parts of the body defended by cuticle without producing any considerable sensation, they must have the same relation to internal parts, and to raw and suppurating surfaces.

The grounds on which he assumes this, his usual eloquent and confident tone, are the well-known facts, that in *emphysema*, the air is driven into all parts of the body of a cellular structure, and unless it advance to such an extent, as to destroy the patient by pressure on the vital organs, may be evacuated by puncture, or absorbed without any unpleasant consequences. The other principal fact, adduced by Mr. Bell is, that the peritoneum is often distended by air in *tympanites*, without being excited to inflammation, or suffering any inconvenience, except perhaps a painful distention.

If Mr. Bell had merely said, that the air possesses no component principle, which is noxious to raw surfaces and

serous and fibrous membranes, the position might have been supported, at least with some plausibility. This is one point I shall attempt to support.

Although the air be perfectly free from any stimulating principle, it is nevertheless injurious, when freely admitted to almost any part of the body, except the cuticle; and the detriment in all such cases is *the effect of the conducting power of the air*—its power to abstract, or conduct off heat and moisture. The air has a very considerable solvent or evaporating power at the temperature of the body; and if a raw surface be exposed to it, the natural moisture of the part is absorbed by the air, the temperature of the part must be affected by this evaporation, and the acrid principles of the moisture concentrated. This loss of heat and moisture, and consequent concentration of the saline or other stimulant properties of the animal fluid, would necessarily produce some change of action in the part; and the action induced would probably be some modification of inflammation.

If this reasoning be correct, we can easily account for the unimportant consequences of *tympanites abdominalis*. The air in this disease being generated in the body, is saturated with moisture, as fast as it is formed: the temperature corresponds with that of the body, and being perfectly circumscribed, there can be no change or succession of fresh portions of air. I would not be understood to imply, by what I have said, that tympanites is unattended with danger; for it is usually the *effect* and *not the cause* of a dangerous condition of the system. But as we are told, that the peritoneum and bowels are quite uninflamed in these cases, we may suppose the air has only a mechanical agency on this membrane, which is distention.

In *emphysema* from a fractured rib, the air passes through the lungs, and is saturated with moisture and brought to the temperature of the body, before it passes into the sac of the pleura, or into the cellular membrane. Besides this, we must observe, that the air is distributed in such minute portions, that without passing through the lungs, it would be saturated with moisture and brought to the temperature

of the body, without inducing any new action. In both these diseases, tympanites and emphysema, the air is so situated as to be, in effect, deprived of its conducting power, and of course would be innocuous.

On the same principle we can explain the effects of air on the synovial and serous membranes. The air detracts so much of the moisture, and affects the temperature so much, as to derange the balance between the exhalents and absorbents, and to excite inflammation.

On this principle we may explain the fact, that inflammation seldom follows paracentesis in cases of ascites and hydrothorax. It is because the exhalents over-balance the absorbents; and there is such a superabundance of the serous exhalation, that the air cannot conduct off enough of it to affect the temperature essentially, or concentrate its saline principles sufficiently to render them too stimulating for the sensibility of the part. In perfect health, it is so difficult to destroy the balance between the exhalents and absorbents, that air, when thrown into the peritoneum, does not always excite inflammation. There is so much activity in the absorbents opening on serous membranes, that they take up, not only large quantities of serum, pus, &c. but likewise atmospheric air.

I am aware of the sentiments of Mr. J. Bell, with respect to the accessibility of the abdominal cavity to the air. His reasoning on this point is in general correct and ingenious; but I would not acknowledge the impossibility, nor even the improbability of its admission under *all* circumstances. I would by no means advance the idea, that the danger of cutting into circumscribed cavities, and especially into joints, arises principally from the access of air. It is most probable, that a wound of their lining membrane, especially if attended with laceration or contusion, would induce inflammation, if air were perfectly excluded; but the free and continued admission of air, through such a wound to such a cavity, would reduce the probability to a certainty. Even in such circumstances, perhaps the air generally excites inflammation first in the wound, which extends into

the cavity. I would agree with Mr. Bell and Dr. Dorsey, that inflammation seldom or never arises in circumscribed cavities from the *sole agency* of air; and the reason is, the difficulty of applying it in such a manner, as to act on the principle I have advanced—that is, so as to conduct off the heat, and especially the moisture, to such an extent, as to destroy the balance between the exhalents and absorbents. Large joints and serous cavities have been laid open and air admitted without inducing any evident inflammation. I saw a case, a patient of my acute and ingenious preceptor Dr. Cyrus Perkins, where the patella was cut across transversely by a blow of an axe, and divided into two nearly equal portions, so that the joint was laid open, and I had a direct view into it. The patient was a healthy boy, aged eight years, and the fragments of the bone united without any apparent affection of the synovial membrane. Cases have occurred, where air has been admitted through a wound into the cavity of the pleura without producing any symptom of inflammation. Dr. Dorsey has related one such case in his *Elements of Surgery*; and in the same paragraph he states, that “in 1790, Dr. Physick injected air into one side of the thorax of a kitten, through a small puncture between the ribs. The wound healed by the first intention, and appeared to occasion very little inconvenience to the animal. On the fourth day the kitten was killed, and not a vestige of inflammation was observable.” Mr. Bell would, perhaps, think a few such facts as sufficient ground for asserting the perfect innocuousness of the air. But on nearly the same ground, we might prove some indisputably poisonous substances to be bland.

In speaking of the power of the *absorbent system*, Dr. Smith of New-Haven says, “if, in a *healthy state* of this system, large quantities of watery fluid are thrown into the cavities of the body, it is soon removed by this system of vessels, and the general health of the system remains unimpaired. So, likewise, large quantities of fluid may be injected into the cellular substance, and quickly removed in the same manner. Even fluids impregnated with poisonous substances

will be absorbed without producing any sensible effects on the system. I once," says Dr. S. "in attempting to inject a sinus on the knee-joint, thence into the cellular substance, more than half a gill of a solution of corrosive sublimate, which must have contained nearly two grains of that medicine. This was soon absorbed without producing any effect on the system."

When we recollect this activity of the absorbent system, and that, as some suppose, the system of brute animals is less prone to put on diseased action than the human, and that Dr. Physick in his experiment threw in so little air, as "to occasion very little inconvenience to the animal," we are not surprised that no inflammation succeeded. It by no means *proves* that there is no noxious principle in the air, much less does it prove that it is indifferent, whether air be allowed a free access to serous membranes and raw surfaces. It invalidates almost none of the hypothesis I am endeavouring to support, viz. that *air is injurious to parts not defended by cuticle, and the injury is the effect of its conducting power.*

This is the most that can be candidly said in favour of the innocuous nature of air, when applied to parts not defended by cuticle, and it would lead to a practice, not differing essentially from that of those, who consider it as possessing some component morbid principle.

But this would be far from meeting the idea of Mr. Bell, for he treats the idea, that "air hurts a wound or sore," as an impression worthy only of superficial and vulgar minds; and for want of argument, he has directed the shafts of his pungent satire against those, who have advanced an opposite opinion. On examining his Discourse on this subject, we shall find more eloquent satire, than sound argument; and that it is better calculated to sway the inexperienced juror, than the wary and discriminating judge. To my apprehension, Mr. J. Bell attempts to disprove a general principle by exceptions, whereas an old maxim says, "*exceptio regulam probat.*"

Notwithstanding Mr. Bell and some others speak so con-

fidently of the perfectly bland and inoperative nature of air, there are others, who maintain, that the deleterious effects of air may be determined by experiments as satisfactory, as any in pathology. I will first adduce the high authority of Bichat's opinion of its effects on membranes, which have no cuticle.

"To the *presence* of the cuticle over mucous membranes, we are," says he, "to attribute *their capacity of being exposed to the air*, and even to the contact of external substances, without exfoliating or inflaming, as we see in imperforated anuses, prolapsus ani, &c. while the fibrous and serous membranes can never bear this contact with impunity." In another part of his *Treatise on Membranes*, he says, "in a living animal, when a serous membrane is laid bare and irritated by chemical or physical agents, the animal rests tranquil; but the action of these excitants (which in a sound state of these membranes is not at first felt,) soon becomes *painful*, if for the shortest time they are *exposed to the air*. All the white organs, the tendons, ligaments, cartilages, in a word, all the parts which Haller called insensible, give little sensation to the animal, by the contact of external bodies, when it takes place in the natural state, when but just exposed; but if the *presence of air* or other cause irritate them, they inflame and become extremely sensible." Speaking of the *tunica arachnoides*, he says, "the pressure of a body, the tearing or cutting action of the scalpel, the application of various caustics, do not seem to excite in the animal any painful sensation; but the membrane is inflamed after a short *exposure to air*."

In these extracts from Bichat, which testify so decisively to the deleterious consequences of the exposure of a defenceless surface to the air, he says nothing of the *rationale* of the agency of the air; and perhaps he would, like Dr. Thompson, attribute it to some component morbid principle. These general facts are all explicable on the hypothesis I have advanced. The application of it to them, would not be difficult; and in doing it, I should be retracing the ground I have recently left.

I shall next quote a paragraph from Dr. Thompson's Lectures on Inflammation, p. 242, from which it appears, that he not only believes in the deleterious effects of air, admitted to parts not defended by cuticle, but that these effects are entirely produced by one of its constituent principles. "The deleterious action of air upon suppurating surfaces," says Dr. T. "has often been denied; but it is now a point which can happily be made the subject of experiment without danger in the human body, as well as in brute animals. To be satisfied of one at least of the effects which air produces, you have only to cover up and alternately expose the slightest wound to and from the action of the air. A certain degree of sharp pungent pain will be felt on each fresh exposure, that will gradually abate, if not entirely cease, soon after the air has been excluded. If the experiment be repeated very often, the edges of the wound will inflame, and a wound, which might at first have been healed up by adhesion, will be converted into one, that can only be healed by the second intention. In this state, that is, of supuration, the accession of external air never fails to occasion pain; and this is an effect, we now know, which depends upon the oxygen which it contains; for this pain may be greatly increased by immersing a fresh wound or suppurating surface in oxygen gas; or not excited at all by opening the wound or suppurating surface in a vessel filled with carbonic acid, azotic gas, or hydrogen air."

The experiments, which lead Dr. Thompson to the conclusion, that the oxygen in the air gives to it its noxious agency upon suppurating surfaces, would seem at first view, to militate directly against the hypothesis, that *it depends upon the conducting power of the air*.

Perhaps this objection can be removed more satisfactorily, than might be at first apprehended.

According to Mr. Dalton, (in his New Chemical Philosophy) oxygen, taken by measure, possesses three times as great a capacity for caloric, as any other gas. It would, therefore, detract three times as much caloric from the part exposed, in order to raise it to any given temperature, that

of the body for instance; and as caloric, in passing from a warmer moist body to a colder, always carries moisture with it, we must *à priori* suppose, that oxygen would be more deleterious than any other gas.

Perhaps we ought not to suppose, that the effects of oxygen would be as much greater, than that of common air, as its capacity for caloric is greater; for the experiment with common air would be performed, where there would be a continual succession of fresh portions of air; while the experiments with the other gases would be performed in small and confined portions. This circumstance, together with their small capacity for caloric, will account for the inertness, which they evinced in Dr. Thompson's experiments. Thus, a strong argument in favour of the hypothesis is derived from the experiment, which seems, at first view, to militate most strongly against it.

I shall next proceed to show, that the practice of physicians bears a much stronger indirect testimony in favour of this hypothesis, than some would at first imagine. In scalds and burns there is the greatest destruction of that defence, which nature has given us against the action of air; and in these complaints, therefore, we might expect to see the agency of the air upon raw surfaces most distinctly characterized; and to find practice, in spite of speculative opinions, gradually settling in a direction that silently, but pretty distinctly, indicates the true pathology. Accordingly we find, that in the treatment of scalds and burns, there has been a great variety of opinions on all points except one. Every grade of stimulus in the *Materia Medica*, from the most bland and soothing to the most irritant—every grade of temperature, from the freezing point to almost the actual cautery, have had their zealous advocates. They agree in one point only; and that is, they defend the excoriated surface against the access of air. The respective advocates of these multifarious and contrary opinions and modes of practice, have each thought there was something specific in his own remedy; but there is reason to apprehend, that it will be found ultimately, that there is nothing

sufficiently specific in any of them to entitle them to that character.

When we find, that remedies, of characters or properties diametrically opposite, accomplish the same object equally well, it seems to me we are obliged to conclude, that the properties, in which they differ, are not essential to them, as remedies; and that the benefit derived from them results from some agency, which is common to all. Most of the remedies that have been employed, "seem," as Dr. Thompson says, "to possess considerable sanative powers," and this opinion is easily reconciled to the theory I have advanced with respect to the agency of air.

The linament composed of equal parts of linseed oil and lime-water, (in Scotland called the Carron oil) has had a high reputation and extensive employment. The linseed is, perhaps, the mildest of all the expressed oils, when it is fresh and pure. It is, however, very liable to become rancid; and it is probable, that the very small portion of lime that enters into this linament, merely serves to correct the rancidity of the oil. Dr. Duncan, Jun. says, this linament may be called a soap of lime, with a great excess of oil; so that, if the lime does any thing more than correct the rancidity of the oil, it can medicate but a small portion of it. It is probable, therefore, that the Carron oil is a very mild defence against the action of the air, and serves as an *artificial cuticle*.

We are told that Mr. Kentish uses the yellow ointment with various modifications in the treatment of burns, *because* it stops the pores of the cloth, impedes evaporation, and confines the effect of the alcohol or spirits of turpentine. He considers it of great importance, that the surface should be left open to the air as little as possible; and he is particular, that such arrangement should be made previous to a dressing, that the exposure to the air may be limited to as short a time as possible.

This treatment, as regards exposure, is precisely such as we should be led to, *à priori*, by the hypothesis I have advanced. The evaporation of these volatile stimulants would

be very rapid from a warm naked surface, which circumstance would render it the more necessary to avoid the exposure of the raw surface. It is stated explicitly, that the principal advantage of the ointment is, that it impedes evaporation, and thus serves one of the most important uses of the cuticle, as a defence against the air. What adds very much to the credibility of this explanation is, that Dr. Physick (as I am told) and other surgeons, find that various other modifications of ointment, with and without spirits of turpentine, are equally beneficial as those of Mr. Kentish. All that seems to be required is, that they should be of a proper consistence, and form a sure defence against the air. This, with some other considerations that follow, seems to lead to the conclusion, that it is immaterial, whether the ointment possess any medicinal properties or not. Heister, after speaking of several remedies, says, "take which you please, the nearest at hand, suppose warm water, burnt wine or spirit of wine, and renew the application frequently. By this means you will find the heat and pain quickly go off," &c. Van Swieten affirms, that he has often seen the pain of a burnt part relieved by the application of warm water. Dr. Thompson says, that "in a number of trials made at different times, he has had occasion to see burns, to which common emollient poultices had been from the first applied, slough and granulate faster and in a more kindly manner, than similar burns in the same persons, to which in some instances the Carron oil, in other instances vinegar, and in others again the oil of turpentine, were applied at the same time with the poultices." He says also, "were he to judge from his own experience, and from the effects which he has seen result from these remedies in the hands of others, he should reckon it a matter of little moment which of those he is going to mention, shall be first employed." The treatment by emollients seems, on the whole, to be his favourite. He regards the application of cloth dipt in olive or linseed oil, as one of the safest applications that can be employed; and they are to be used for the very same reason I would assign, viz. "they cover and defend the burned parts from

the action of the air ; they admit of being removed with ease, or without being removed, they can be moistened with fresh quantities of oil."

M. Larrey "recommends dressing all deep burns with fine old linen, spread with saffron ointment, which," he says, "has the property of diminishing the pain, and *preventing irritation by keeping the nervous papillæ from coming in contact with the air*, or being pressed by the linen and clothes." He speaks confidently of the great success of this, which he calls soothing and gently tonic treatment.

It will be found on examination, that the applications, which afford the best safeguard against the influence of air, constitute the essential part of the practice ; indeed, it seems to be the only point, that is pursued without deviation through the cure. For the advocates of the stimulant practice are compelled to abandon their favourite remedies in the course of a few hours, except in those cases, where the injury is so slight, as to get well in spite of interfering remedies. I hazard the conjecture—and I have some confidence in the opinion—that ere long very much of that confidence which has been, and still is reposed by many, and perhaps I may say by most of our intelligent practitioners in the *stimulant practice*, will be destroyed. It will undoubtedly be long adhered to by those, who feel a personal pride in adhering to early opinions, and who feel, on the renunciation of an opinion, which they have once advocated, as if they had relinquished a portion of their integrity. But let them look forward into futurity, enter the cemetery of once venerated, departed opinions, and perhaps they will find reposing side by side on the same shelf, the practice of dressing fresh wounds with heating balsams, and that of treating burns with hot stimulants. A practice, which has been introduced within a few years, adds very much to the credibility of the opinion I have hazarded, with respect to the agency of air and the treatment of burns. I refer to the practice of enveloping the injured part of the patient in raw cotton. It is now, by many respectable physicians, particularly in the Eastern States, esteemed the most eligible practice.

It is sufficient to remind you of the ample favourable testimony, which Dr. Harris gave to it at our last meeting.* I have never heard a *rationale* of its operation; but I venture to assert, that it possesses no peculiar medicinal qualities, but merely prevents the access of air; prevents evaporation; regulates the temperature of the part; being so soft also, as to present no mechanical irritation: thus combining the advantages of plasters and poultices, but devoid of some of their inconveniences.

Undoubtedly some will contend, that a remedy so salutary must possess some medicinal virtues, quite peculiar to itself. This would be a supposition altogether gratuitous; for, in the first place, its good effects can be accounted for, without supposing such a property. In the next place, it is strange, that an article in such common use, for every kind of apparel worn next the skin, and which has likewise been so much employed in surgical dressings, should not be discovered to be medicinal, until applied to burns. M. Larrey uses lint, and were it made as soft, and could it be as easily procured, it would be in no respect better or worse than cotton. Perhaps the ultimate fibres of the lint are better conductors

* He stated before the Academy of Medicine, and has since made the same statement to me in private conversation, that in the action between the Algerine frigate and the *Guerriere*, a gun burst on board the latter vessel, and between thirty and forty men were burnt. One half of them were treated with cotton, and the other half with spirits of turpentine and basilicon ointment, according to the directions of Mr. Kentish. The patients were so equally divided, both in respect to number and to the degree of injury, that the experiment was a very fair trial of the comparative value of the two modes of treatment. Those who were treated with cotton recovered much quicker, and with less suffering than those who were treated in the other mode, and the experiment was in every respect decisive in favour of the treatment by cotton. Dr. Hoffman was the surgeon on board of the *Guerriere*, and stated the facts to Dr. Harris.

Dr. Harris informs me, that he has used the cotton in many instances in his own practice, and has no hesitation in giving it preference to the stimulant practice. In discussing this subject before the Academy, he was not prepared to admit, that a treatment so efficacious as he had found this, depends upon properties so negative, as I ascribed to cotton in my *rationale* of its effects. It operated so like a charm in alleviating pain, that he thought it must possess some specific medicinal virtue. It is now too late, however, to resort very frequently to the doctrine of specifics, in order to avoid the trouble of devising a *rationale*, or to give a kind of "right divine" to a remedy.

of caloric, than those of cotton ; but the difference would be so trifling, if any, as to be unessential as a remedy.

Soon after this paper was read before the Academy, I found in the Philadelphia Journal for May, p. 200, an extract from the London Medical Repository for Jan. 1821, which goes far to confirm me in the sentiments I have advanced, with respect to the agency of air. From that it appears that the continued application of moisture at the temperature of body, prevents or allays pain and inflammation, and that mere lint and water are as good in the subsequent treatment as the most powerful medicaments. Now the whole ground of my objection to the admission of air to wounds and raw surfaces was, that it *excites inflammation, or some new action, principally by abstracting the moisture of the part*, and, which is an effect of this, by *reducing the temperature* ; but I suppose that the application of air saturated with moisture, and at the natural temperature of the body, would be innocuous.

ART. VII. *An Essay on the Devastation of the Gums, and the Alveolar Processes.* By L. KOECKER, Esq. Dentist, Honorary Member of the Philadelphia Medical Society, &c.

THIS disease, I suspect, is far from being accurately understood, and is generally supposed to be incurable. The remarks which I am about to offer on it, are the result of close practical observation, and as the subject is of no inconsiderable importance, they may not, perhaps, prove either useless or unacceptable.

In his Natural History of the Teeth,* Mr. Hunter says, "How far these diseases can be prevented and cured, is, I believe, not known." Fox† differs very little from Hunter on this point. He speaks of the affection in so vague a manner, and so much so, indeed, of its cure, that I am inclined to believe he never saw one effected, at least

* Vol. i. p. 51.

† Natural History, &c. part ii. p. 95.

when it had advanced to any considerable extent. At all events, I feel persuaded that the subject has not been considered with sufficient attention by those writers who have come under my notice.*

This disease I have met with in its different stages, and have always succeeded in curing it by the mode of treatment which I am now to describe.

Cases of this nature have their beginning generally, in an inflammation and suppuration of the gums, which gradually extend to the periosteum of the alveolar processes, and of the teeth: or they begin by an inflammation of these parts, which is afterwards communicated to the gums. They very rarely originate in the alveoli themselves.

The inflammation and suppuration are sometimes violent, and the absorption goes on rapidly, while in other instances, it is so slow in its progress as to be scarcely perceptible, and the suppuration destroys the gums in a very gradual manner, attended by the absorption of the alveoli and their periosteum. The teeth now losing their support, become loose and drop out successively.

The disease seldom attacks all the alveolar processes at once, but generally begins at some one part of the mouth. I have seen it commence at the incisores, though more frequently at the molares.

The crowns, necks, and particularly the exposed parts of the roots, are frequently covered with a greenish glutinous substance, and with adhering tartar. The spaces between

* The late Dr. La Roche informed me, that he had seen this disease very frequently in the French colonies, and in persons of different ages,—that its progress was very rapid, and that it was generally considered peculiar to the climate, and altogether incurable.

In 1812, during my short practice in Lancaster, I saw a gentleman aged about forty-five years, who had been deprived of the use of all his teeth from this disease. He showed me about twenty which were tolerably sound. He mentioned that eight years had passed before the whole were lost; during that period he suffered much pain, and enjoyed neither peace nor comfort till he was deprived of every tooth. He was so contented with the relief which the entire loss of his teeth occasioned, that my proposition to supply him with a set of artificial ones, was rejected without the least hesitation.

the teeth are filled up with a tartar of a dark brown or greenish colour, but sometimes of the usual appearance, and in other instances, I have even seen them so clean as to deceive a superficial observer, but a close examination has invariably found some tartar adhering to the roots, and pressing upon the alveolar processes, hid under the edges of the gums, and in the spaces between the teeth.

The gums are, for the most part, much swelled and inflamed, attended by a thin pus-like discharge from between them and the roots of the teeth, occasioning an offensive smell. These parts sometimes exhibit a pretty healthy appearance, though never, as far at least as my experience extends, perfectly free from inflammation, as Hunter and Fox have stated.

As already intimated, this disease frequently in the beginning, proceeds very slowly, and often affects only certain parts of the gums for many years. When once, however, it becomes general, it goes on with such rapidity, that while one tooth is dropping out, others are so loose, that the slightest pressure almost, will make them fall from their sockets.

Persons of robust constitutions are very liable to this affection of the gums, and it shows itself oftener after the age of forty than any other time. Their teeth are many times perfectly sound, or very little affected with caries, though I have occasionally met with them considerably diseased and painful.

Among other remote causes of the disease, may be enumerated a scorbutic and scrofulous habit affecting the gums; the frequent and inordinate use of mercury; great irregularities in the position of the teeth; neglect of cleanliness; smoking and chewing of tobacco; operations of different kinds injudiciously performed, such, for example, as the filing of the teeth, when the general state of the mouth, from an imperfect healthy action, is not capable of bearing the unavoidable irritation which it occasions, or, where too great violence is used in this operation, even in a healthy state of the mouth. Certain tooth powders may also be

mentioned, as charcoal, from the irritation which it mechanically excites in the gums.

By the above causes, a collection of tartar, without which I have never seen this disease, is deposited upon particular parts of the teeth, and this becomes the immediate or exciting cause of the disease, and so long as it is suffered to remain, entirely prevents the success of such efforts as nature may make for the accomplishment of a cure.

When the disease has advanced considerably, and some of the teeth have already become much exposed and loose, they prove another, and a more powerfully aggravating cause. The disease having, therefore, proceeded thus far, it may be considered as near its height, since the teeth will now fall out one after the other, till they are all lost.

The mode of treatment consists in first checking the diseased action which has become habitual, and in producing a general healthy disposition in the diseased parts, and those connected with them, by removing the exciting causes: and secondly, in preventing their recurrence.

The first indication is realized by extracting such teeth as are too much affected by the disease to be capable of permanent restoration, and by a complete removal of the tartar and glutinous matter covering the remaining teeth and their fangs, which produce the irritation in the alveolar processes. The second indication is met by removing such remote causes as may still exist, or, if this cannot be done, by counteracting their influence. When of a general character, such as from scurvy, scrofula, or the effects of mercury, constitutional remedies combined with topical means are required. Being local, however, that treatment to which I now beg leave to draw the particular attention of the reader, is only demanded. The remedies prescribed for the removal of the causes mentioned, are of very great importance. Considerable irregularity of the teeth existing, such teeth as excite, from their unnatural situation, a constant irritation, either in the opposite teeth or upon their gums and alveoli, should be removed, and thus the effects of their irregularities prevented. Operations injudicious-

ly performed, require such remedies as will remove the injurious effects resulting from them. The individual having been entirely neglectful in cleansing his teeth, or where this has been done in an improper manner, more careful attention under the direction of a skilful dentist to these circumstances, will be demanded, in order to prevent the re-accumulation of tartar.

With the foregoing measures may be combined some astringent or tonic washes, such as the diluted tincture of Peruvian bark and myrrh, which I have found very serviceable.

Considerable experience and judgment are required to discriminate at once, between those teeth which may be capable of preservation, and such as ought to be extracted.

The principal and fundamental curative means in the disease of the alveolar processes, are the removal of teeth so much injured from the effects of the disease, as to have become a direct exciting cause to its further progress, as well as of those which are at the moment, from a variety of circumstances, indirect causes, and must unavoidably become, in the progress of time, sources of the irritation and morbid condition of the surrounding parts. The disease, therefore, being so far advanced as to render it necessary to extract some of the teeth, this operation must precede every other remedial attempt, since, omitting this, a cure is impossible, and all other operations not only prove fruitless, but have an immediate tendency to increase the irritability of the affected parts, and the violence and rapidity of the disease itself.

It is hence a matter of considerable moment to establish some guide, as far at least as the intricacy of the subject will allow, by which we may be directed in deciding upon those circumstances that ought to induce us to extract the teeth, or to make efforts to preserve them. In such an estimate, the weight which a number of considerations of a minute and incidental character should have upon the determination and conduct of the operator, must be left very much to his own judgment and experience.

The incisores and cuspidati being the most important teeth, the main attention is always to be directed to them. They are, fortunately, from their natural situation, and from their peculiar formation and functions, not only the most favoured, but they are besides, capable of suffering more from this disease without being destroyed, than the other teeth. They may, indeed, be preserved, even when not more than one half of the alveolar processes remains, provided, that a considerable portion of the fang continues united to the alveoli by its periosteum. I would always endeavour to preserve them unless they were very loose. More, however, than one half of the alveolar cavity being absorbed, and where the tooth is very loose, or has lost its vitality, it must be extracted for the benefit of those adjoining.

The bicuspidates, and molares, are much less favourable for preservation than those just mentioned, and the process of mastication imposes on them a greater mechanical irritation. They are also more inconveniently situated and formed, and generally have several roots, in consequence of which, the difficulty of keeping them free from tartar is not a little increased.

They should be extracted: First, when they are very loose, as I consider teeth in this state a direct cause of disease, from the constant irritation which they occasion to the alveoli: secondly, when the alveolar processes have receded from them so much as to expose the spaces between the fangs of the tooth, since then it will be impossible to prevent the reaccumulation of tartar in these places, which inevitably continues the disease, and from which such teeth are to be considered its indirect causes. The suppuration cannot fail to be sustained by the irritation of the tartar, until the tooth becomes loose, and is then, itself, both a direct and indirect exciting cause of the disease.

No one of the molares should be permitted to remain, where it has no antagonist, particularly if it is situated in the upper jaw. The utility of such teeth is destroyed for want of an opposing surface to act upon, and they have therefore, an influence upon the surrounding parts not un-

like an extraneous body. As for instance, should there be by accident an interval between two antagonists when the jaw is closed, nature uniformly attempts to remedy the evil. The cavity of the alveoli in the first place contracts at its bottom, and drives out the tooth until it meets its opponent, by which the necessary stimulus is restored, and the morbid action cured. If a tooth is lost entirely, it produces in a similar manner the destruction of the one opposite. This long struggle of nature at last, will involve in a greater or lesser degree, all the other teeth in a like morbid condition. It is necessary, therefore, to end it by extraction.

Every tooth that has lost its vitality, as well as every stump, should be taken away, and also those teeth, which, from an irregular situation or direction, excite a mechanical irritation.

Teeth to be removed, must be extracted with great skill, and all at the same sitting, because it would almost obviate, or at least greatly lessen, the desired effects, if the operations were performed at different intervals. This advice is the result of much experience, and merits, I think, some consideration. The bleeding from the sockets should be encouraged by warm water taken into the mouth at short intervals, during the different operations, and continued for some time, as the effects are of much importance. A gentle astringent wash may be subsequently employed for several days. I have generally found the following simple preparations the most useful.

R	Mel Despumat.	℥ iij
	Acid. Vini.	℥ j

This diluted, in the proportion of three table-spoonsful to a pint of warm sage tea or water, and used frequently during the day.

R	Mel Despumat.	
	Tinct. Cinchon.	ā ā ℥ ij
	Mix and dilute as above.	

R	Mel Despumat.	
	Tinct. Myrrh,	ā ā ℥ ij
	Mix and use as above.	

R Mel,

Tinct. Catechu, ā ā ȳ j

Mix, dilute and use as above.

In about ten or fourteen days after the removal of the teeth, the inflammation is found considerably subsided, the gums assume a more healthy appearance, and the teeth grow in a corresponding degree firmly in their sockets.

The tartar should now be removed, and there is often much difficulty in doing this well. It adheres so firmly to the necks and roots of the teeth, which are generally more or less loose—it is attached so closely to the alveolar processes, and is in many cases so covered by the edges of the gums, and so hid between the teeth, that it is not without the utmost care we are able to avoid irritating the gums and periosteum with the instruments employed. The operation on this account, is to be performed with the greatest caution and delicacy, in order as well to remove the tartar perfectly, as to avoid making the teeth still more loose.

Instead of believing that the removal of all the tartar at the same time can endanger the teeth by exposing them, as asserted by several writers, it seems to me when it can be effected, the sooner they are relieved from such a pernicious coat, the more we contribute to their preservation. Yet we are in this case, oftentimes under the necessity of contenting ourselves with the removal of a certain portion at once, and finish the remainder after a lapse of eight or ten days. As the tartar is most injurious in those parts where it presses upon the alveolar processes, it is highly necessary that every particle of it should be removed.

After the stony matter, or hard tartar, has been taken away, the removal of the green mucus is still an important part of the operation. This cannot be effected with instruments. It may be done by dipping a piece of thick sole-leather or soft wood into some dissolving tooth-powder or liquid, and then rubbing the surfaces of the roots and crowns of the teeth with it, to be followed by the use of a soft brush. The dentist's care is now to be directed to the prevention of the re-accumulation of the tartar. Not only

on this, does a perfect cure almost entirely depend, but also the prevention of its recurrence. To effect this purpose, however, with satisfaction, is found quite an arduous task.

There is always much predisposition to form new tartar, and the difficulty of preventing it from settling on the teeth and fangs, is augmented by the great irritability and tenderness of the gums, the debilitated state of the periosteum, and the inflammation in the alveola.

The frequent repetition of scaling the teeth with instruments, cannot prove a substitute for not keeping them clean. It has on the contrary a tendency to maintain great irritation, and the benefit derived in this way, without proper care afterwards, can only be temporary.

Mechanical means alone are, for the most part, either insufficient or too violent, and powerful chemical means are apt to produce such diseases of the teeth, as will destroy them in another way. The only method, therefore, which remains, is to use these means combinedly, by which the bad effects may be avoided, and the advantages of both obtained. The rule to guide us in applying these remedies, is, the further the disease is advanced, the more the chemical agency is indicated, and the less is mechanical power to be applied, and conversely.

Having scaled the tartar from the teeth with instruments, I adopt the following plan. I request the patient to brush his teeth in the morning, and after every meal, with an astringent powder, possessing a sufficiently dissolving power upon the tartar, and with a brush not so hard as to create much pain. After this process has been continued for about four weeks, and the accumulation of tartar found to be diminished, I direct the powder to be used in the morning and evening only, and the brush and water after every meal. The brush should neither be too hard nor too soft, and so formed as to clean the spaces between the teeth, and to give that mechanical stimulus to the gums which is so highly beneficial.

The internal gums are considerably harder and less irri-

table than the external, a fact which has not been noticed, at least as far as my observation extends, by any writer.

Tartar accumulates much more rapidly upon the inner, than on the external side of the teeth, and hence a different brush for each surface is required. That for the internal side should be made of the best hog's bristles, and cut from five-eighths to three-quarters of an inch in length: while that for the external, is to be made of the best loose hair, and cut about half an inch long at the commencement, and as the disease decreases, it may be used a little shorter and harder.

The patient should be directed to press as hard with the brush as he can bear against the gums and teeth, so that its bristles may insinuate themselves into the spaces of the teeth, and between the edges of the gums and the roots of the teeth. The pressure of the brush is to be applied in the direction from the grinding surface of the teeth towards their roots, so that the mucus which adheres to the roots under the edges of the gums may be completely detached.

The mode of brushing the teeth which I direct to be adopted, is entirely the reverse of that recommended by every writer on the subject whom I have consulted. They all advise, that the brush should be applied from the gums towards the teeth, the consequence of which is, that the tartar remains fixed close to the alveolar processes, and that some mucus is pressed into the spaces at each brushing. The intention they have in advising the brush to be thus used, is to excite the gums to grow over the roots again: but so far from this being effected, much injury is sure to follow this practice. There is a natural tendency in the edges of the gums to get their proper place, provided every thing that can interfere with this effort be carefully and constantly removed.

By Mr. Hunter, very good effects are expected from scarifying the gums repeatedly—and Fox places still more reliance on the advantages of this operation. To me it has always appeared as affording only temporary relief, and to be of no real benefit in permanently arresting the progress of the dis-

ease. It should, I think, be discarded, since, though palliative in its immediate tendencies, it will occasion delay in the use of a more effective treatment, and therefore must, in its ulterior consequences, be considered as absolutely pernicious. By the extraction of one tooth whose removal is indicated, we do more good than by fifty scarifications—and if the disease be not so far gone as to require the extraction of any teeth, the immediate removal of the tartar has the same effect. If there is any inflammation in the gums, the mere pressure of the instruments on them in scaling the teeth, must unavoidably occasion some bleeding, which, together with the removal of the exciting cause (the tartar) answers more directly and more permanently, than any scarifications for the removal of the inflammation.

The practice of fastening such teeth as are very loose, by ligatures to their contiguous teeth, as recommended by Fox, is still more injurious, and should never be resorted to, even where a radical cure is no longer to be looked for.

Teeth which are fit for preservation, will grow again by restoring the tone of their periosteum and the other surrounding parts—and this is to be effected by the remedies already indicated. The ligature not only prevents the loose teeth from growing firm again, but also tends to loosen those to which it is attached, for the following reasons. The gold wire, however thin and pure, has not sufficient flexibility to permit the loose teeth to grow in their natural situation, or to be applied without some irritating pressure on the gums and adjacent teeth. The ligature made of silk has the inconvenience that it will contract when it becomes wet with the saliva, and thereby impose a constant straining upon the teeth.

The operations which are necessary to be performed on decayed teeth, such as plugging, filing, or cutting out carious parts, must be deferred until the parts have acquired health and vigour to support the irritation which they cause.*

* I have always delayed such operations for at least three months. An attempt too early to perform them, may not only bring on a relapse of the disease, but would probably prevent the successful results of the operation.

Artificial teeth should if possible be avoided, and only made use of under imperious necessity, and prepared with the greatest care and accuracy, in order to prevent any injurious mechanical influence upon the gums and those teeth which come in contact with them.

CASE.

On the third of May, 1818, Mr. P. from Baltimore, consulted me on the subject of his teeth. On examination, I found that he was labouring under the affection of the gums already described. Nearly half of the alveolar processes of the incisores and cuspidati of both jaws was destroyed. Those of the upper, rather more than those of the lower. The teeth were a little loose. The alveoli of the second molares of both jaws had suffered almost as much as those of the incisores. The latter were not loose, but the partitions of the alveolar cavities between the roots were so much absorbed, that I could pass a probe between the roots. Those of the upper dentes sapientiæ had not suffered so much, and the antagonists, the two under dentes sapientiæ, were the only teeth which the patient had lost. The alveoli of the bicuspidæ and of the first molares had suffered less than any other. Their roots were about one-eighth of an inch exposed. The gums were slightly inflamed, and there was a small discharge of a whitish matter all around their edges, from between the fangs of the teeth, and the edges of the gums.

The patient had always paid a good deal of attention to cleanliness, and there was, therefore, not much tartar on the teeth, yet they were not entirely free from it, and particularly the fangs, to which there was a considerable portion adhering close to the alveolar processes. The teeth were all tolerably healthy, and but one had been painful. They were well and handsomely formed, and regular in their position, with the exception of the first under bicuspid on the right side, which was pressed outwards and acted unnaturally upon the opposite cuspidatus and first bicuspid.

The gentleman was about thirty-two years old, and of

an excellent constitution, and had never to his recollection, been sick even a single day.

About three years prior to his consulting me, he applied for advice to one of the most respectable dentists of Paris, who, after examining his teeth, told him that his mouth was labouring under the consequences of venereal disease, and advised him to apply to a physician. The gentleman, however, assured him that this was impossible, as he never had the slightest taint of a venereal character. The dentist persisted in his assertion, and told him that the state of his gums rendered his opinion altogether certain, whatever the patient might declare to the contrary. Disgusted with such ignorance and presumption, the gentleman left him, but neglected to take any further advice.

Several weeks before I saw him he was forced, from a violent tooth ache, to apply to a dentist in Baltimore, to relieve him from this distressing affliction. The dentist, however, refused to extract the tooth, alleging, that its removal was unnecessary, as the patient would, in a few years, lose all his teeth.

The gentleman forgot his pain, left the dentist by whom he was much alarmed, and resolved to consult me on the subject, which intention was immediately communicated by letter.

When I saw him, I conceived that I hazarded nothing by the assertion, that I had very great hopes of preserving all those teeth which were the most important. I found him perfectly willing to submit to any plan of cure which I should see proper to adopt. I therefore extracted the two upper dentes sapientiæ, which had lost their antagonists—the four second molares, the roots of which were so much exposed as to preclude every hope of success in attempting to save them, and the first bicuspid of the right side, the irregularity of which has been noticed. Nothing but warm water was freely used until the next day.

When I saw my patient again, he was entirely free from pain: the bleeding which succeeded the operation had removed the inflammation of the gums, and he felt so per-

fectly well, that I did not hesitate to comply with his request, to proceed on the next day with the operation, as he was anxious to return to Baltimore. Conformably to this design, I removed the tartar carefully from his teeth, and on the subsequent day, he left the city to return to his home.

I was afterwards informed that he was in good health, but I did not see him again for some considerable time. The disease was entirely arrested, his teeth had grown firm, and the gums were free from all inflammation. From the absorption going on in the alveolar processes of the extracted teeth, however, he had not been able to keep them perfectly free from tartar, and the second operation of scaling his teeth became necessary, and which took place about one year after the first occurred.

His teeth were perfectly clean and the gums and alveoli quite healthy in the month of September, 1819, when one of them was plugged with gold, and the carious parts of the incisores cut and filed out. I provided him with tooth powder, and requested him to observe rigid exactness with regard to cleanliness. Since that time I have repeatedly heard from him, and without the least complaint.

ART. VIII. *On the Modus Operandi of Medicines.*

By N. CHAPMAN, M. D.

ON this very intricate question, not a little difference of opinion prevails. The only point in the controversy which seems to be conceded, is, that the operation of medicines does not depend on any of the common laws of matter, but, on a principle incident to vitality alone.

“*Medicamentum, non agit in cadaver.*”

As this is so universally admitted, as even to become one of the established maxims of the schools, it may be right before we advance further into the discussion, to endeavour

to fix our notions in relation to the nature of life. But as speculations of this sort, indulged to any extent, would be here misplaced, I shall exhibit my views of the subject, in as narrow a compass as possible.

Of the various doctrines of vitality, one only appears to me to be at all well founded, and consequently deserving our attention. It presumes that every animated body, animal or vegetable, is endowed with a *primordial principle of life*, and which, resident in the ova of animals, and the seed of plants, constitutes the power by which, in the first place, the various organs are moulded, developed, and perfected, and by which, afterwards, the animal economy is defended against the action of mechanical and chemical laws.

Located, perhaps, in the highest degree, among the digestive and assimilative organs, it enables them to change, or destroy the qualities of the substances exposed to their operation, without sustaining in return the slightest injury or change. It would hence really appear, that instead of matter, whether aliment, drink or medicine, acting on the living system, as is commonly imagined, it is on the contrary, the living system which operates on these matters. But such is the case only, when the vital energies are in a vigorous and healthy condition. Different, indeed, is the result, where from debility or other imperfection, the vital organs are rendered unfit to act upon substances, or of resisting the action of substances on the system. Whatever is taken into the stomach under such circumstances, preserves its properties unaltered, or undergoes the same sort of alteration, which it would do out of the body, or beyond the sphere of the vital powers. Each article in this state, obeys the order of its affinities, and the changes which occur, are purely chemical. Common matter, now acting by its own laws, the system being thus languid and decayed, sinks under an attack it cannot repel, and the processes of fermentation and putrefaction ensue, which, if not timely arrested, become the precursors, as well as the causes of death and destruction.

Life, therefore, may be defined the principle, or power by which the system preserves its own integrity unimpaired,

and its several parts from decomposition, amidst the action of surrounding agents, while it acts upon things foreign to itself, assimilates them to its nature, and appropriates them to the supply of its exigencies, or to the redress of its injuries.

The theory I shall propose, of the operation of medicines, alleges, that they all act by exciting a local impression which is extended through the medium of sympathy. By many, however, it is still believed, that certain articles, at least, enter the circulation and produce their effects in this way.

This latter hypothesis is evidently a relict of the humoral pathology. By the disciples of that sect, it was held, that disease mainly consists in a depravation of the blood, "from too great tenuity or viscidty, by an excess of acid or alkaline acrimony, by morbid matter entering from without, or generated within."

As a necessary consequence of such notions, medicines were supposed to penetrate into the circulation, and by a sort of chemical action to correct the vitiated condition of the fluids, and hence the origin of the terms, inspissants, attenuants, antacids, antalkalies, antiseptics, diluents, demulcents, &c. &c.

As they are now pretty generally exploded, to demonstrate the fallacy of these speculations, by any very minute detail of facts or reasonings, cannot be required. All changes in the condition of the fluids are wrought by impressions made through the intervention of the solids. Not the slightest proof exists, so far as I know, of their undergoing any mutations, either by spontaneous action, or from the introduction of foreign matters, much less that such is the cause of disease, or the mode in which remedies operate.

To reach the circulation, medicines must pass either by the lacteals, or lymphatics. Now it seems more than probable, in either case, their powers would be so neutralized by the preparatory processes of animalization, as to be deprived of all activity.

Can it indeed be credited, that any substance, after a subjection to the digestive and assimilative powers, retains in

the slightest degree its original properties? Experiments, on the contrary, show, that chyle, however diversified the materials may be out of which it is formed, whether animal or vegetable, has essentially an identity of nature, and instead of being a *crude*, as is commonly imagined, is in reality a highly *elaborated* fluid, having many, and perhaps all the properties of blood, except its red colour. Three of the constituents of blood it at least contains.

1. There is one portion of chyle, which preserves its fluidity during life, but coagulates after death, by exposure to the air, and is probably fibrine.

2. There is a second portion, which resembles serum, in continuing fluid when exposed to the atmosphere, and in coagulating at the same degree of temperature as serum.

3. There is a third, consisting of globules, similar to those of blood, with this difference only, that they are much more minute.

The fact of the perfect and uniform constitution of chyle seems to me, at once, to put down the hypothesis which I am combating. Yet, perhaps it may be said, it proves nothing in the case of medicines administered otherwise than by the stomach, as when applied to the surface of the body, or introduced into the bowels. To this objection the answer is obvious, and I think very satisfactory.

No one who has carefully attended to the phenomena of the absorbent system, can help admitting, that every section of it is endowed with the power of *digestion* and *assimilation*, and the lymphatics quite as conspicuously as the lacteals. This capacity is given, as a provision of nature, to exclude noxious matters from the circulation.

The absorbents, in most instances, are fully adequate to this end, and when they are not, the substance penetrates to the first conglobate gland, which takes on inflammation, and arrests its further progress, these organs acting here as centinels, guarding the exterior approaches of the body. That some of the properties of certain articles are displayed in the secretions and excretions, I am not disposed to deny. But it does not hence follow, that these substances entered

the circulation in their primitive state. Directly the reverse indeed seems to be proved, as no one of them can be detected in the serum of the blood.

To me it is clear, that the process of assimilation, as performed either by the chylopoietic viscera, or by any part of the absorbent apparatus, completely decomposes all substances, and however discrepant in their properties, reduces them to a homogeneous fluid fitted for the purpose of nutrition. But when thrown into the secretions or excretions, being removed beyond the control of the vital energies, chemical affinities are sometimes again brought into play, by which these substances are in part, or wholly regenerated.

No slender support is given to this hypothesis by the well known fact, that matters are found in such positions, which had not previously existed, in any cognizable state, in the blood; and furthermore, that certain articles can only be detected in certain fluids, as the odour of garlic in milk, of asparagus in urine, of sulphur in the perspiration; and we may add, as another illustration of the same import, that the colouring principle of madder can be traced in no part of the solids, except the bones, and their immediate appendages, the cartilages. Did these articles pre-exist in the blood, instead of being regenerated in some such manner as I have stated, ought they not to be thrown out indiscriminately by all the emunctories?

Whether this explanation be received or not, it must at least be acknowledged, that no substance, in its active state, does reach the circulation, since it is shown, that a small portion even of the mildest fluid, as milk or mucilage, oil or pus, cannot be injected into the blood vessels, without occasioning the most fatal consequences.

Twenty-two years ago, in conjunction with my friend, the late Dr. George Lee, then resident in the Pennsylvania Hospital, I instituted a series of experiments, to this purport. All the articles enumerated above, were tried in succession, together with some others of an acrid and stimulating nature, on dogs and cats, the animals selected on the occasion.

But diversified as these substances are, we could discern no material difference in their effects, the whole seeming to act merely as extraneous matter in *error loci*, producing, at first, great distress to the animal, as was indicated by its movements and cries, followed by difficult panting respiration, vomiting and purging, nervous tremors, convulsions, and death. Experiments very analogous to the preceding, have recently, I understand, been made by Professor Caldwell, and with confirmatory results.

That the late inquiries of Sir Everard Home and others, lead to a different conclusion, I am aware. Confiding, however, in the accuracy of our own observations, I must, in the present state of the question, still maintain, without the slightest qualification, the position I have assumed. The fact is, that though the blood be alive to impressions, and, perhaps, even more exquisitely than the solids, it being designed, that all matters should be excluded the circulation, they lose their specific mode of action, when introduced, and cause confused and disordered movements, like an irritant in the trachea, or in any other unnatural situation.*

Conceding, however, to the humoral pathologists all which their doctrine demands, still insuperable difficulties remain in the way of its adoption, to account for the operation of medicines. Not to dwell tediously on the subject, I shall content myself at present with little more than merely mentioning that we are not at all informed by it, why our remedies, after mixing with the blood, should be directed to one organ in preference to another, as mercury to the salivary glands, or how indeed they operate at all.

By the *medication* of the blood, were it possible, as is contended for, we must in all instances do harm. The whole mass of circulating fluids is equally charged in this case with the medicinal substance, and therefore, while a salutary action is going on in the diseased organ, every sound

* On the experiments of Dr. Majendie, on venous absorption, I forbear to comment, as on repetition here by Dr. Sommerville, they were not confirmed. The subject is now before the Academy of Medicine, where it will receive the fullest investigation.

part of the system becomes subjected to a similar impression, which could not fail to disturb the order of health, and create morbid derangements.

Be it alleged, as it sometimes has been, that the action of medicines, under such circumstances, is on the surface of the blood vessels, the doctrine becomes utterly deserted, and we are forced to recur to sympathy, as affording the only explanation.

By a recent writer of high authority, whose opinions however on this point, are not wholly without the taint of the humoral pathology, it is conceded, as an incontrovertible fact, that a large proportion of medicines do act by the "medium of nervous communication."

"This," says he, "is manifest, from the effects of these substances being produced in a shorter time after they have been received into the stomach, than they could be, were they to act by being absorbed with the chyle into the circulating mass. The stimulus of wine or opium will instantly remove lassitude, and increase the vigour of the circulation, or of muscular exertion. Digitalis given in sufficient quantity, very speedily reduces to a great degree the frequency of the pulse, or a large dose of cinchona, exhibited half an hour before the expected recurrence of the paroxysm of an intermittent, will prevent its attack."*

Exactly the same conclusion is deduced by Mr. Brodie, from his experiments in relation to nearly all the poisonous substances, vegetable as well as mineral; and the few which he represents as acting otherwise, really, from his own showing, seem not to do so.†

The principle being thus clearly established in so large a number of instances, which, if necessary, might be still further increased, it appears to me, that it should be admitted as an universal law, unless exceptions to it are very clearly made out and demonstrated. To multiply causes superfluously, is against one of the fundamental rules of

* Murray's Mat. Med.

† Philosophical Transactions of the Royal Society, for 1811 and 1812.

philosophising, and is not less repugnant to the general course of nature, whose means are proverbially distinguished by great simplicity and uniformity.

As regards the mercurial preparations, an example particularly selected by the writer whom I first cited, to illustrate the occasional admission of medicines into the circulation, we have the most conclusive proof that this never happens, whatever may be the manner in which they are employed,* and no doubt such is the case with all the articles of the *materia medica*. It is at least as clearly shown, by experiments made by myself many years ago, and which have been since more than once repeated with greater precision, and on a more extensive scale, by graduates of this School, that none of the preparations of iron, of copper, of lead, nor the colouring matter of indigo, of madder, or of rhubarb, can be traced even so far as the chyle.†

Being introduced into the small intestines of dogs, these several articles were observed to be rapidly taken up by the lacteals, the coloured ones losing their tints in passing through the conglobate glands, and in every instance, so completely were their properties obliterated, as not at all to be cognizable by any chemical test in the contents of the thoracic duct.

Yet, it is not to be concealed, that some very respectable writers have affirmed, that certain matters do enter the circulation, and may be detected by chemical tests. Of the facts of this kind, not the least relied on, is one which comes recently from Mr. Brande. It is stated by this most eminent chemist, that the blood of a patient who had taken soda, long and largely, was found to contain that alkali in excess.

Granting it to be true, is not the force of the fact done away by the recollection, that soda is a constituent of blood, and as such, may freely enter into its composition without detriment. All the experiments, however, on this subject,

* Experiments of Drs. Physick and Seybert, *Medical Repository*, vol. v.

† Hodge's *Inaug. Dissertation*, in which many of these experiments are contained.

have been made without adverting to a source of fallacy, which greatly impeaches their validity. On blood dead and separated into its parts, have the whole been made, and obviously under such circumstances, the elements of things may again re-combine, in a manner already noticed, which in the living state were broken up, and assimilated by the powers of animalization. *

Could more proof be required of the operation of medicines being entirely independent of the circulation, it might be found in the well ascertained fact, that many of them produce their full effects, though the heart and blood vessels be previously removed. Long ago it was shown by Whytt, that if the heart of a frog is taken out, and a solution of opium be injected into the abdomen, the animal speedily becomes convulsed. The poison of the viper, according to Girtanner, applied to a frog prepared in the same way, will destroy it as soon as if no mutilation of the animal had taken place. Numerous experiments of a similar import, with other active substances, have been instituted, the results of which are so exactly correspondent, that to detail them would be superfluous. But, before I finally dismiss this part of my inquiry, I will only remark, that the converse has been equally proved, or that no extension of the impression of medicines happens in cases where the brain and spinal marrow have been destroyed, though the heart and vascular system be preserved uninjured.*

It results, on the whole, from what I have said, that we are to reject the fluids altogether in our inquiries relative to the operations of medicines, because, in addition to the reasons already stated for doing so, we have in that law of the animal economy termed sympathy, or consent of parts, a solution of the problem which comports infinitely better with the existing state of our knowledge.

Conformably to the theory I have adopted, whenever a medicinal substance is applied to a susceptible portion of the body externally or internally, an action is excited, which

* Murray's Mat. Medica.

is extended more or less, according to the diffusibility of the properties of the substance, or the degree of sympathetic connexion which the part may maintain with the body generally. Thus a set of actions is raised, every one of which is precisely similar, provided they are confined to the same system, by which is to be understood parts of an identity of structure. If, however, the chain runs into other systems, it loses its homogeneous character, the actions being modified by the peculiar organization of the parts in which they may take place. These are principles of universal application. In every case, whether it respects the operation of remedies, or the production of disease, the spot primarily acted upon is a point, from which is diffused the radiated impressions. This is a mode of action peculiar to living matter, and is remarkably distinguished from all the other processes. An impression is made and extended without mixture or combination, or in any degree disturbing the order and constitution of the part in which it takes place. But in chemical operations, to which we must look for an explanation, in the event of rejecting the above rationale, decomposition inevitably occurs—and as the result, new compounds must be formed from a union of the elements of the part, with the substance applied, wholly subversive of the existing structure. By a course of medicine, and, indeed, by every meal, our nature would become essentially changed. Exceedingly preposterous as it may seem, such is the direct and legitimate corrolary, from any other than the sympathetic view of the *modus operandi* of agents on the animated frame.

Let it not, however, be supposed, that I am among those who consider the blood as a dead fluid, without any immediate dependencies or connexion with the rest of the system. Every argument, on the contrary, which can be adduced in favour of the animation of any section of the solids, strikes me as being equally applicable to the circulating fluids.

Referring to the writings of Mr. Hunter, for a full development of the evidence to this effect, I shall now, only state, that such a property in blood, is essentially necessary to the preservation of life. Between *living*

and *dead* matter, there is an eternal hostility, which, under all circumstances, proves repugnant, and utterly irreconcilable. The moment a part dies, an effort is made by the conservative powers of the constitution, to throw it off, as we see in the processes of sloughing and exfoliation: where it is not successful, morbid irritations commence, in illustration of which we have many well known facts, and particularly the case of hectic fever. Even the secreted fluids cannot remain long in their proper receptacles without doing mischief, as is perceived in retention of urine, though the bladder be not painfully distended, as well as in other similar cases.

The same effects precisely would take place, were the blood destitute of vitality: instead of the source of all action and life, it must become a morbid irritant, productive of inflammation, or what is more probable, of immediate death. These views derive no inconsiderable support from what happens in the transfusion of blood. Experiments originally made by Dr. Physick, and subsequently repeated by Mr. Blundell, of London, demonstrate, that prior to the coagulation of blood, or in other words, till it dies, which takes place, for the most part, in about thirty minutes from the time it is drawn, it produces no bad effects. But after this, being injected, it destroys life as speedily as any dead or extraneous fluid whatever.

The animal machine is constituted of solids and fluids, by the latter of which, I mean the blood. It is a whole made up of parts, which, though somewhat dissimilar, and existing in different proportions, are no less endowed with the vital influence, and held together, and made to harmonize by common susceptibilities and sympathies.

Connected indissolubly by these ties, an impression made on any one portion of the system, whether solid or fluid, is equally felt by both, on account of this established consent of parts.

Do we not find the blood variously affected in diseases, so much so, indeed, as really to form not the least unerring criterion of the nature of the case? Be this admitted, can

we require any further proof of the correctness of the preceding views? It is indeed impossible to conceive how the blood can undergo such changes, from impressions on the solids, were they not united by sympathy, an exclusive attribute of the living state. No one has pretended to show, that they are of a chemical nature, or analogous to the changes which take place after death—and I think, I have rendered it pretty certain, that they cannot be ascribed to the introduction of any foreign matter into the circulation. It remains for me, on this point, only to observe, in further illustration of my general principle, that so close is the consent between the blood and solids, and so completely reciprocal their dependence, that impressions made on the former, as, among many other instances, by a particle of the virus of the viper, death instantly ensues, and the whole animal fabric, solids and fluids, present the spectacle of a broken texture, and total disorganization.

There would seem, at first, to be here some inconsistency with what I have previously stated on this part of my subject. But I think a little care in the comparison of the passages, will relieve me from this imputation, without the trouble of any explanation.

Entertaining such opinions, it may, perhaps, be demanded, why I reject the circulation, as a medium of the operation of remedies, and as a primary seat of disease? My answer to this inquiry might be collected, with no great difficulty, from the strain and scope of much of the preceding reasoning. It will be recollected, that I have strenuously endeavoured to prove that no article, whether remedial or morbid in its tendencies, enters the blood with a retention in any degree, of its original or active qualities, and hence, that the fluids can only be secondarily affected, through the mediation and by virtue of the sympathies which they maintain with the solids.

As the doctrine here advanced is intimately connected with the principle of sympathy, it may be proper that I should say a few words on this subject. There are, indeed, not wanting some who have affected scepticism as to the

very existence of such a law. It must be confessed, at present, we have no very distinct intelligence relative to its nature. But are we, on this account, to question its existence? Equally might we doubt of the sensibility or irritability of the body, since neither of these qualities of vital matter has been precisely demonstrated. Notwithstanding this, we are persuaded of their existence from the phenomena which they exhibit, and it is by the same description of evidence that we are, or ought to be, assured of the existence of sympathy.

“*Causa latet vis est notissima.*”

In employing this term, therefore, I mean only to denote, like chemical affinity, caloric, and many other such expressions, a principle, or power, of which we know nothing except from the experience of its effects, the precise essence or nature being occult, and concealed. Thus, in the same way, says Newton, “What I call *attraction* may be performed by *impulse*, or by some other means unknown to me. I use the word here to signify only in general, any force by which bodies tend towards one another, whatever be the *cause*.”

Of the manner in which impressions are extended, as well as of the cause of the more intimate consent of parts, we are not perhaps accurately informed. It would seem, however, that in neither case is it to be exclusively referred to the nerves, as is commonly supposed. Those sympathies which prevail among the various viscera of the abdomen, and between them and the head, neck, and contents of the thorax, may be explained with sufficient probability, by the extensive anastomoses of the intercostals with almost all the nerves which proceed from the spinal marrow. But, there are many other sympathies, not less conspicuous, between parts, the nerves of which have not the slightest connexion. It appears, that either by the co-operation of different organs in the performance of a function, as in the complex apparatus subservient to respiration, or from similarity of structure, parts, though detached, being prone to be affected by the same cause, as the parotid gland and testes in the

male, and the same gland with the mammæ in the female, the habit of acting in unison is acquired, and sometimes confirmed. This habit of concerted action is termed association, and has been adopted as a principle by Locke, by Hartley, and by Darwin, to account for the connexion in many of the motions of the body, as well as in the operations of the mind. Both the sound and morbid states of the system present numerous instances of these associated actions, some of which are constant and uniform, while others are occasional and anomalous, produced, as it were, accidentally.

The principle of sympathy pervades the body, every portion of it being susceptible of associative actions, by which means the several parts are linked together so as to constitute one whole, or a unity of system. It is to this principle, whatever it may be, which, uniting all the organs of the animal economy, we are to impute the wonderful concurrence and perfect harmony which is observable in its complicated actions, during health. But though this general medium exists, to the reception and propagation of impressions, there are three surfaces on which remedies, and perhaps the causes of disease, more particularly operate. These are,

1. The alimentary canal.
2. The skin.
3. The organ of smell.

Each of these parts has a considerable susceptibility, and maintains a very extensive connexion with the system generally, though the stomach is possessed of infinitely the quickest sensibility to action, and the most intimate and multiplied relations. No viscus or organ, not even the brain itself, can be compared to it, in this respect, or which occupies so important a station in the animal economy.

Destitute of a stomach, no animal can exist. Life may be sustained, even in the perfect animals, independently of almost every other organ. Examples are numerous of fœtuses being born of a full size without a brain, spinal marrow, heart, lungs, liver, or uterus. No instance, how-

ever, has been met with, in the course of my very extensive researches on this subject, where the stomach was wanting. As the most indispensable of the vital organs, it seems, indeed, to be an inseparable incident to every variety and gradation of animal existence. No matter how inferior the being may be, it is always provided with some apparatus equivalent to a stomach.

Conveniently situated for the purpose, the stomach is probably the throne of the vital principle, from which would seem to emanate an influence that, diffused over the system, preserves, as I have already mentioned, the order of the parts, and sustains the vigour, tone, and well-being of the whole animal economy.

“*Languido ventriculo, omnia languent.*”

Assailed, however, by impressions which it cannot resist, this organ, as the centre of association, becomes the seat of the first link in the chain of most diseases, and is always the chief medium of the operation of our remedies, in the correction of morbid derangements.

As a continuation, pretty much of the same structure as the stomach, the intestines afford also a medium by which medicines may be introduced, and sometimes, with great advantage. There is, indeed, scarcely one article of the *matéria medica* which cannot be so managed, as to produce its full effect when thus employed. To attain this, however, the dose should be largely increased, and, as a general rule, about three times the quantity is demanded. It is, perhaps, not sufficiently known, that after the stomach, by long use, has lost, or had greatly impaired, its susceptibility to the action of a medicine, this will operate with fresh and unabated force, if applied to the rectum.

That the surface of the body is another part on which our remedies act, has been known from the earliest times, and the practice founded upon it, is probably to be ranked among the first attempts that were made, in the infancy of our science, toward the removal of disease. The whole of the cutaneous surface seems endowed with some sensibility to impression, though the soles of the feet are possessed of

it in the largest degree, and hence, have been considered by a distinguished teacher of physic, as constituting, among the widest avenues to the invasion of disease, or for the introduction of remedial impressions. But, though the practice has been so long and generally pursued, it was never doubted till lately, that when thus applied, medicines operate in any other way than by entering the circulation. Even the rubefacient and vesicating applications were, at one time, not excepted: the effects of these, being ascribed altogether to the absorption of the acrid particles of the substance used for these purposes, into the blood, and thereby raising the excitement of the system.

Nearly about the same period, Mons. Seguin, in France, and Dr. Rousseau, of this city, called in question the existence of cuticular absorption. But the praise of laborious and successful investigation, is due exclusively to the latter experimentalist.

As early as the year 1800, it was proved, or at least rendered highly probable by him, that the *pulmonary organs*, and not the *skin*, constitute the inlet through which certain substances enter the system.

By cutting off all communication with the lungs, which he easily effected by breathing through a tube protruded into the external atmosphere, he found, that though the surface of the body were bathed with the juice of garlic, or the spirit of turpentine, none of the qualities of these fluids could be detected, either in the urine or the serum of the blood.

Conducted nearly on the same principle, but with a greater diversity of substances, experiments exceedingly well devised, and neatly executed, have since been made by persons of opposite prepossessions, to an almost incredible extent. Contradictory as many of these are, a candid examination of the whole, will still lead to a pretty satisfactory conviction, that absorption from the surface of the human body does not exist as a natural and ordinary function.

Borne down with the weight of evidence against them, most of the advocates of the ancient hypothesis were indeed

prepared to abandon it, as no longer tenable, when about four or five years ago, an experiment made by Dr. Mussey, again revived their faith in cuticular absorption. He very clearly proved, that if the body be immersed in a decoction of madder, the colouring matter of this substance will be taken in, and may be displayed in the urine by using any one of the alkalies, as a test.

Determined, if possible, to put this agitated question to rest, Dr. Rousseau, assisted by his friend Dr. Saml. B. Smith, has subsequently performed a series of experiments, many of which I witnessed, and can therefore bear testimony to their accuracy, with every variety of substances, mild and acrid, volatile and fixed, nutritive, medicinal, and poisonous.

The result of these extensive researches, is:

1. That of all the substances employed, madder and rhubarb are those only which affect the urine; the latter of the two, the more readily enters the system. Neither of these articles can be traced in any other of the secretions or excretions, or in the serum of the blood.
2. That the power of absorption is limited to a very small portion of the surface of the body. The only parts, indeed, which seem to possess it, are the spaces between the middle of the thigh and hip, and between the middle of the arm and shoulder.

Topical bathing with a decoction of rhubarb or madder, and poultices of these substances applied to the back, or abdomen, or sides, or shoulders, produced no change in the urine, and equally ineffectual was immersion of the feet and hands in a bath of the same materials, which, after being kept in it for several hours, not the slightest proof of absorption was afforded.

As I have described, such is the state in which this interesting subject is at present left. Though, perhaps, not absolutely decided, enough surely has been done, to demonstrate, that cuticular absorption rarely happens, and that whenever it does, it cannot be deemed the effort of a natural function.

Covered, as is the whole surface of the body, by the impervious cuticle, it is manifest to me, that absorption can alone take place in one of two ways: either by forcing the substance under the scales of the epidermis, as in the instance of the application of frictions, or by continued bathings, or fomentations, the cuticle becomes so changed in its organization, as to admit of transudation, or the insinuation of the fluid under its squamous structure, so as to come in contact with the mouths of the lymphatics situated within.

At all events, whatever difference of opinion may be entertained as to the degree of conclusiveness of the experiments to which I have alluded, it cannot be necessary to resort to cuticular absorption to explain the effects of substances applied to the surface of the body. We shall do this much more satisfactorily, by referring it to sympathy, and to another source which I am presently to point out.

That the skin has a very intimate connexion with the body generally, and more especially with the stomach, is a fact so notorious, that it would be a waste of time to attempt to prove it. It is through this medium that most substances applied to the surface, certainly operate.

The discovery of the pulmonary absorption, would seem, I confess, on the first view, to militate in some degree against this opinion, and to render it more than probable, that volatile matters, at least, are inhaled and act through the lungs. Experiments, however, have recently been made,* which go far to invalidate this supposition, and show, that under such circumstances, it is the olfactory nerves which are the seat of the impression, and the medium through which these volatile matters produce their effects. These experiments would occupy too much space to be separately detailed. Collectively, they warrant the conclusion, that, by simply closing the nostrils, either by compression by the fingers, or by filling them up with putty, the fumes of ardent spirits, of a strong decoction of tobacco, or an infusion of opium, may be inhaled for one hour, without any

* By Dr. Rousseau.

unpleasant effects: whereas, if the precaution mentioned be omitted, the consequences are proved to be most distressing.

New as these results are, and inconsistent with our pre-existent notions as they may be, they are rendered highly probable, independently of the respectability of the source whence they proceed, by some facts of a very striking and indisputable nature. Every practitioner has witnessed how powerfully all the volatile and odorous matters operate on the olfactory nerves in health and in sickness: and it is hardly less known, that when the sense of smell is impaired by a coryza, or entirely suspended by obstructing the nostrils, that the sensible qualities of most substances are so lost, that they cannot be accurately discriminated, and this extends, even to those articles of our food or drink, with which we are most familiar. The preceding facts are sufficient, at least, to awaken curiosity on this subject, and to urge to more exact inquiries, by which the truth may be elicited and confirmed.

(To be continued.)

ART. IX. *An Account of the Ziziphus Vulgaris, and Ziziphus Lotus, or the Large and Little Jujubes.* Communicated by WILLIAM P. C. BARTON, M. D. &c.

LETTERS have recently been received from the American consul at Tunis, by his friends in this city, stating that he has been cured by an Arabian physician, (said to be the Bey's physician,) of an obstinate teterous or cancerous affection of one of his eyes. This gentleman had been several years under the care of our first surgeons in this city; and, though at times temporarily relieved by their skill, finally sailed for Tunis, labouring under the same obstinate affection of his eye. The letter which I have seen from the consul on this subject, attributes his cure chiefly to a plant which he calls "*petit jubabe*," though he does not state in what manner it was applied, or what part was used—

and by his ordering a present of the best surgical instruments which could be procured here, to be sent to his physician, that his future patients might have the benefit of "sharper instruments than he had," there would seem an intimation that some kind of operation had been performed. However this may be, the consul desires to know if the "*petit jubabe*" is a native of this country, mentioning his intention of sending it hither, in case it should not be, that barbarians should not alone be possessed of so invaluable a plant. As the flora of that part of Africa where we are told the plant is indigenous, is well known, I have carefully searched the French botanical and medical works for the "*petit jubabe*," but have not been able to succeed in finding any vegetable described by such name; neither do any of the travellers into Africa describe any such plant. Hence I am led to conclude that it is *le petit jujube*, the *smaller jujube*, which grows particularly abundant in the kingdom of Tunis. Under a belief that this is the plant intended, I subjoin a botanical and medical history of this and the larger jujube, with the view of attracting the attention of the surgical and medical profession to the subject. And I do this the more willingly, on account of the interest which the cure has excited among medical men in this city, to many of whom the case of the consul was well known; and he had, preceding his departure, been under the care of our distinguished and skillful surgeon, the professor of anatomy in the University of Pennsylvania.

The plant alluded to by the name *petit jujube*, is the *ziziphus lotus* of Willdenow, who thus describes it: *Ziziphus lotus*, aculeis geminis, alterno recurvo, foliis ovato-oblongis, obsolete-crenatis. It is the *rhamnus lotus* of Linnæus, the *ziziphus sylvestris* of Tournefort. Under the latter name it is described by Shaw, (Africa, 631, f. 631,) and under the Linnæan name—by Desfontaines, in act. Paris, for 1788, 446, t. 21, and by Mungo Park, (Travels, 99, with a plate.) Prickles in pairs, one of them hooked. Leaves elliptic-oblong, slightly crenate, three-ribbed, smooth on both sides. It is a native of Africa, especially in the kingdom of Tunis. We

learn from Shaw, that it is common "in a tract called *Jareed*, which was formerly the country of the *Lotophagi*. The Arabs know this plant by the name of *Seedra*. It has the habit of a *rhamnus*, and the *flowers* of the common jujube. But the *fruit* is smaller, rounder and sweeter, the size of sloes, with a large stone. This *fruit* is borne on every part of the branches, like gooseberries; whereas that of common jujube grows only on the slender annual shoots, thrown out from the ends of the branches. The *Z. vulgaris*, is twenty feet or more in height, with a large furrowed *stem*, twisted *branches*, knotty at the extremities, and large oblong *leaves*; but the *lotus* is scarcely three or four cubits high, with numerous shoots from the same root, which are smoother, straighter, and paler, or whitish; the *leaves* small, round, and more rigid. The *fruit* is ripe and fit for eating in December and January." Such is Dr. Shaw's very intelligent account; to which Dr. Smith adds, "that the *prickles* grow in pairs, both of them very straight, slender and sharp when young, but in process of time, one becomes thick and hooked, the other much elongated, remaining quite straight. The *leaves* are at most an inch long, perfectly smooth, naked and green on both sides; their three ribs sometimes separate, sometimes more or less combined."

This is a plant of ancient celebrity. Homer speaks of it in his *Odyssey*, where he says its fruit is so delicious, that it causes strangers to lose a recollection of their native country. It grew abundantly in the country inhabited by the ancient *Lotophagi*, the *Gerbi* of the present day, where it is even at this time very common. It is frequently met with, indeed, in most of the countries on the coast of Barbary. It grows in thickets, and is scarcely four or five feet high. The branches are tortuous, slender, of an ash-white colour, garnished with prickles; the leaves resemble, a good deal, those of the common jujube, but are smaller, more round, and three-nerved; the fruit smaller, more round, of a reddish colour, and sweet flavour, tolerably agreeable, but very inferior to their ancient reputation. The fruit is sold, to this day, in the markets. The Arabs on the borders of the little

Syrte, and of the neighbourhood of the desert, eat them, and also nourish their cattle with them. They also make a drink of them, by bruising and mixing them with water. Polybius and Theophrastus have both described this plant, and nearly such, according to their accounts, were the uses the ancients made of the fruit.

Ziziphus vulgaris, of Willdenow, *Rhamnus ziziphus*, of Linnæus, is the common jujube—called *jujubæ majores oblongæ*, by Bauhin. *Ziziphus*, by Tournefort. Jujube Arabum, sive *ziziphus Dononæi*. It is the jujubier of the French; giuggiolo, of the Italians; azufayfo, of the Spaniards; maceira de anafega, of the Portuguese; brustbeere, of the Germans; jujube tree, of the English; jobenboom of the Hollanders; onnab, of the Arabs; unap, of the Persians and Turks; unabi, of the Georgians; hum-hao, of the Chinese; naatime, of the Japanese.

This is the greater jujube, and, according to Pliny, it is a native of the coast of Syria, whence it was carried into the countries of Italy, in the reign of Augustus. It has been long since naturalized on the borders of the Mediterranean, in Provence, Languedoc, Barbary and the Levant. It grows in form of a large shrub, attaining a height of fifteen or twenty feet. The stems are very branching, tortuous, covered with a brownish bark, somewhat divided by chinks. The branches are brownish-red, flexuous, garnished at their base with two prickles, the one straight, the other bent like a fish-hook. The leaves are alternate, sub-petiolated, margined, stiff, oval, slightly toothed, and marked by three longitudinal nerves. The flowers grow in clusters, from the axills of the leaves. The calix consists of five oval segments—petals shorter than the calix, narrow, hollowed in the form of a cup—filaments shorter than the petals—anthers roundish. The fruit is a pulpy drupe, of a reddish colour, of the form and size of an olive, containing a stony nut of two cells, one kernel in each cell.

Poiret has described these two species botanically, as above, but the medical and dietetic properties of the fruits of both appear to be considered by E. P. Chaumeton,

Chamberet and Poiret, as identical. They describe these properties as appertaining equally to the fruit of *le petit jujube* and the "*jujubier*" or common jujube. The following is their history of these fruits. "Under a red pellicle, which opens after the maturation of the fruit, the jujubes enclose a whitish parenchyma, which is soft, pulpy and succulent, and which becomes spongy by desiccation, when it acquires a vinous and saccharine taste, in place of the sweet slightly acidulous flavour, which it possessed in its fresh state. Chemical analysis has not yet made known the relative proportions of the immediate components of these fruits. The presence of sugar is always detected in them, together with a great quantity of a soft and viscous mucilage, soluble in water, which leaves no doubt of their being susceptible of yielding alcohol by distillation." According to the French writers just quoted, some authors have thought they recognised in these fruits, the *στεφίνα* of Galen, so much so, that Bauhin thinks that they are nothing else than the fruits which Athenius, Theophrastus, Pliny, and other ancients have mentioned, under the name of *lotus*. However this may be, they state that "these fruits are mucilaginous and saccharine, and very nourishing; and are manifestly endowed with the emollient, demulcent, relaxing and lubricating properties, which appertain to all mucilaginous fruits, particularly figs and raisins. According to these different accounts, they may be successfully made use of in the treatment of most of the inflammatory diseases; and in acute and chronic affections accompanied with dryness, heat and irritation. A decoction of them in water has been extolled, as a remedy for diseases of the chest—as pulmonary catarrhs, hoarseness, phthisis, and irritation of the throat. They may be employed with equal success against inflammatory affections of the larynx, (*phthisie laryngée*) angina and aphtha, diarrhœa and dysentery, whether in the form of drink, or a more substantial form. According to the well-known benefit of all mucilaginous solutions in nephritis, the decoction may also be administered with success in this dis-

case, in inflammation of the bladder, in gonorrhœa, in calculous affections, and other diseases of the urinary canals.

As demulcent and as nutritive, the decoction of the pulp of jujubes is really advantageous when the stomach can retain it, as in hectic fever and in certain obstinate and continued diseases of the skin, accompanied with heat and irritation, such as *tetters*, elephantiasis, prurigo, &c. But whatever may be the advantages of these fruits in the largest proportion of the cases just enumerated, we are not disposed, say Chaumeton and Chamberet, to allow them greater efficacy than dates, figs or raisins, which ought to be preferred, because the jujubes are often considerably mouldy, wormy, or sour, when they arrive in Europe.

In the fresh state, these fruits constitute a very nutritious aliment, which is tolerably agreeable and salutary. The people of the middle of Europe eat them in this state, after they have been dried by exposure to the rays of the sun. When perfectly dried, they are put up in boxes, and constitute an article of commerce. Thus prepared, the jujubes are very saccharine, but they are at the same time much more solid, and more difficult of digestion, which renders them less proper dietetic articles for delicate persons, who live a sedentary life, and whose digestion is impaired.

Jujubes are administered in decoction of water or milk, in the dose of from thirty-two to sixty *grammes*, (one to two ounces,) to one kilogramme (two pounds) of liquid. The syrup which is prepared from it, so highly extolled as a remedy for pulmonary affections, may be administered with the same success as a decoction of the fruits, but it has no other virtues than those of the marsh mallow, (*malva officinalis*.) These fruits enter into the composition of a *restorative syrup*, an *elixir of health*, a *lenitive electuary*, and other medicines reported to possess pectoral virtues. Their mucilage may be made into a paste; and lozenges of jujubes have a taste which is as agreeable as their effect is salutary.

From this account of the medical virtues of the jujubes, it would appear, that the cure of the obstinate affection of

the eye already mentioned, and which we are informed, has been effected by one of them, *le petit jujube*, was produced by the kind effect they are stated to have on inflammatory affections. And, as it has been stated, that the disease of the eye alluded to, was of a teterous nature, and as the jujubes are known to remove obstinate affections of this kind from the skin, it appears very probable, that the cure has in reality been accomplished by *le petit jujube*; but in what manner it was applied, we are not informed. From what the French authors say of the comparative usefulness of the jujubes and figs, raisins and dates, we could hardly be led to expect greater benefit from them in inflammatory affections of the eyes, than from the pith of sassafras, which we know proves the best of our mucilaginous and alleviating collyria; yet, full force should not be allowed to the qualifying remarks on the remediate virtues of the jujubes, made by Chaumeton and others, since they refer to these fruits in their dried state. It is not unlikely that *le petit jujube*, in its native country, and while quite fresh, is possessed of properties which are lost by desiccation. Whatever be the truth on this point, the fact of the cure, and our knowledge of the nature of the remedy employed, afford proof of the usefulness and efficacy of mild mucilaginous and emollient applications in affections of the eye.

We possess in the United States, an indigenous species of the very natural genus to which the jujubes belong, viz. the *ziziphus volubilis*, (*rhamnus volubilis* of Linnæus.) It grows in deep swamps near the sea coast in Virginia, the Carolinas and Georgia; delighting in damp, rich, clayey soils. It is known by the name of *supple-jack*. It grows abundantly in the Dismal Swamp of North Carolina, where it attains the height of the tallest bald cypress trees, (*cupressus disticha*.) The known similarity of medical virtues possessed by plants like the present, belonging to the same genus and same natural family, all of the known foreign species of which appear to possess a similitude of virtue, would naturally lead us to look for some useful medicinal properties in *supple-jack*. I am not possessed of any infor-

mation relative to this plant other than its botanical history, which I here subjoin, to enable any physician in the vicinity of its native site, to investigate its properties.

Ziziphus volubilis, unarmed, leaves ovate, ribbed, very entire, slightly undulate, umbels on peduncles, axillary, stem twining. Sp. pl. A climbing plant, stem twining over trees and shrubs, sometimes to a great height. It has many small branches. Leaves alternate, oval and ovate, acute, glabrous. Flowers borne in small umbels or racemes, axillary and terminal, dioicous. Drupe oval, of a purple colour, generally containing one seed. Mr. Elliot observes, that in the structure of the leaves, and in its mode of flowering, it is very nearly allied to *rhamnus carolinianus*. He states, that the stem, when green, is very flexible and tough.

It is, perhaps, needless to remark here, that one species of the genus *rhamnus*, to which the jujubes and the supple-jack formerly belonged, has long since been an accredited article of the materia medica, viz. *rhamnus catharticus*, or purging buck-thorn.

Philadelphia, July 13th.



ART. X. *On the Tincture of the Poppy, made from the Entire Plant, as a Substitute for Laudanum.* By DANIEL WILSON, M. D.

IN reading the interesting remarks of Dr. Duncan, Sen'r, of Edinburgh, on the preparation and use of the tincture of lettuce, I was led to suppose that a tincture of the *papaver somniferum* would prove a substitute for the *tinctura opii*, as well as for the celebrated gum from which that tincture is made. This idea presented itself on first reading Dr. Duncan's remarks about eighteen months since, and last summer I availed myself of an opportunity to subject my hypothesis to the test of experiment. Except the radix, I used every part of the plant, a quantity of which was obtained at

the proper age for procuring the extract. In the eagerness of my pursuit, I dried the first parcel in the sun, but I was at once convinced that this would disappoint my views. A second parcel was procured and carefully dried in the shade, and having reduced it to a coarse powder, I proceeded with the intention of making a saturated tincture: the following is the result of my experiments.

℞ Pulv. pav. somnif. $\frac{2}{3}$ iv.
sp. communis $\frac{2}{3}$ xvj.

Mix, digest for eight days and filter.

From these quantities I obtained eight or ten ounces of the tincture, which I forthwith introduced into my practice. Under the same principles which govern me in administering the tinctura opii, I gave the tinctura papaver: and in doses of double the quantity which I give of the former, I find the latter an effectual anodyne.

I am about trying a tincture made with the sp. vini, but my experiments are yet only in embryo. From tinct. pav. $\frac{3}{4}$ i. as above prepared, I procured extr. gr. xvij., but when formed into pills, they run together, unless given immediately, and are therefore fit only for extemporaneous use. I have laid aside the tinctura opii for some months past in my practice, not because I think it inferior to the tinct. pav. but because I believe the latter to be quite its equal as an anodyne, and when considered in an economical point of view, its advantages are too obvious to require enumeration.

Professional pursuits, together with an unfortunate habit of procrastination, have prevented me from making this communication sooner. I hoped too, that more time would have enabled me to make it more worthy of the attention of the faculty: but as it is now uncertain whether I shall be able for some months to add to, or diversify my experiments, I have resolved, imperfect as it is, to commit my project to the hands of others.

Louisville, June 18th.

CASES.

ART. XI. *Cases of the Gradual Loss of Muscular Power, without evident cause.* By JAMES MEASE, M. D.

THE occurrence, in the year 1818, of a distressing case of the gradual loss of voluntary power in the muscles, (and which has not yet finished its course,) forcibly brought to my recollection a similar one which I occasionally saw in 1807, in a most worthy man, who finally died, and who at the time strongly excited my sympathy, from the Job-like patience, and christian resignation, with which he bore his severe affliction. Other reasons served to impress this case forcibly on my mind. These were the singularity of the fact, that no cause could be assigned for the origin of the disease: my never having seen, heard, or read of a similar case, and the declarations of every member of the profession to whom I mentioned it, that nothing of the kind had ever been met with by them. While watching the progress of the second case, I saw in a New-York paper, a notice of the death of a gentleman on Long Island, from a disease substantially similar; and about the same time I met with the interesting account of another, by Dr. Bostock, in the ninth volume of the London Medico-Chirurgical Transactions, (1818); and shortly after a third case fell under my notice. On the supposition that the recording of them may be useful, in leading the faculty to a consideration of the subject, I send them for insertion in your Journal.

J. M.

July 8, 1821.

CASE I.

Mr. — Lawrence, a commission merchant in Philadelphia, aged forty years, after being greatly fatigued by attention to business, was seized with a nervous fever, by

which he suffered severely, in the year 1804. His previous habits of life were temperate and correct. About a year after his recovery, a weakness and uneasy sensation in his left arm and wrist were perceived, and continued until the next year, when he became affected in like manner in his right arm. He now applied for medical aid. A blister to his left arm produced no relief, but brought on a great pain in his bowels,* which was relieved by flaxseed tea. The chalybeate spring and pure air of Schoolley's Mountain in New Jersey, were tried without the least benefit. More generous diet than usual, with the addition of Madeira wine at dinner, the daily exercise of the muscles in lifting weights, frictions with a stimulating oil, the use of the cold bath in summer, and warm bath in winter, all which were prescribed by Dr. Rush, were equally inefficacious. Frictions and electricity, produced a temporary suspension of his uneasy sensations, but no permanent benefit. In spite of every thing that could be tried, the loss of power of the muscles and general debility increased, until he was obliged to remain in bed. His speech then became affected, and finally he lost the power of swallowing. Until that time, his appetite continued good. After a gradual waste of the powers of life, from increase of the original disease, and the want of nourishment in consequence of his inability of deglutition, he expired in the year 1810, to use his brother's expression, "like the snuff of a candle."

CASE II.

The next case that finished its career, and which I am to relate, occurred in the person of the late Mr. Berrian Petty, a respectable citizen of Stonybrook, Long-Island, New-York. For this I am indebted to Dr. B. F. Thomson, of Setauket, L. I. who obtained the particulars from Dr. Grif-fing, the attending physician of the sufferer. The complaint commenced in the spring of 1816, and appears to have

* I use the words of his brother, who gave me the particulars of the case. The pain probably proceeded from the strangury arising from the blister.

made its approach in the most gradual manner. The principal inability was in the right leg and foot for the first ten months, during which time Mr. P. used to observe, that he was often angry with himself for a careless practice of tripping his toes when walking. The foot gradually curved inwardly and shrunk ; the muscles became flabby and soft, and occasionally he complained of rheumatic pains about the knee-joint. At this time, however, the power over the muscles above the knee was perfect: the case appeared similar to those of children whose limbs waste while at the breast. Blisters were freely applied to the leg, and issues to the sacrum. The next spring he suffered in a similar manner in the left leg. He then went to New-Haven, and consulted the faculty there ; issues were made by caustic over the sacrum, and kept running six weeks ; the shower bath, warm and cold water alternately ; bandages spread with stimulating plasters, from the toes to the hips ; rubefacients, frictions, electricity, several calomel purges, pelltory tooth-tincture and powder in large doses ; tincture of rhus vernix, and daily exercise in a carriage, were all successively given and tried without stopping the gradual progress of the disease. In the autumn he went to New-York, where the actual cautery was used along the course of the spine, from the cervix to the sacrum : nux vomica was also given in tincture, and the dose increased until it produced extensive eruptions, and very much disturbed the functions of the stomach.

From this time, say November, 1818, he gave up the use of medicines, except palliatives. The legs became useless ; the arms, in the mean time, had taken on the diseased action : still the nerves preserved their powers of sensation, and the heat of the limbs was as great as usual. The stomach and other viscera performed their functions regularly until late in the disease, when probably owing to the want of exercise, and the change from a very active life to one of an extreme sedentary nature, he became dyspeptic. From the symptoms under which he laboured, a disease of the spine was suspected, and hence the remedies abovementioned

were tried, but he never complained of pain or any weakness in that part; nor during life, nor after death was it possible to ascertain the cause or seat of the disease. His intellectual faculties remained perfect to the last, nor was his virility in the least affected until his bodily weakness became excessive. His face wore the aspect of health until within a few weeks of his death, being but little emaciated. From long confinement, and a total state of helplessness, his anguish was extreme; no change of posture afforded relief; rest forsook his pillow, and he expired in a sitting posture, on the 21st July, 1819, aged forty-three years.

CASE III.

Hugh Donahy, at the age of fifty-two, that is, in the year 1811, was seized with a severe rheumatic affection in his right arm, in consequence of its being exposed to a cold rain, while riding in a stage coach in Ireland, on his way to the sea-port at which he intended to embark for the United States. Owing to the extremely crowded state of the vessel, he suffered in common with the rest of the passengers, from want of accommodations; and on their arrival on the banks of New-Foundland, they were visited by two British government ships, the officers of which, finding that there were more passengers on board than were allowed by law to vessels of a certain tonnage, took possession of the vessel, and sent her to Loughswilly in Ireland, where she was condemned and sold after due process of law. Donahy then went to Belfast, and again embarked for the United States, where at length he arrived, after having been eight months on board ship. During both passages he experienced a numbness and pricking sensation in his right arm, and a few months after reaching Philadelphia, the same sensation extended to his right leg, and he became sensible of a want of command over the muscles of his right lower limb. This induced him to go into the Pennsylvania Hospital for relief, and after remaining a patient there nine months, was so far relieved as to be able to act as clerk in a store. The numbness and pricking sensation, although alleviated, yet

still continued in a slight degree, and some time after, he was seized with a severe palpitation of the heart, and a want of command of the abdominal muscles when at stool. The pricking and numb feeling then began to extend to the left arm. Still, however, he was able to write, but he dragged his leg heavily. About eight months since, (Oct. 1820,) he was suddenly seized while walking, or rather shuffling along, with a dizziness that occasioned him to fall; he soon, however, recovered and stood up; but the want of power over the muscles of his limbs, which were in continual gentle agitation, except when held, or pressed against a table, became so great as to oblige him to give up his employment. About two years since he felt a cold stream descending from his right knee to his right foot, down the sides of the leg, while the rest of the leg and foot felt as when in perfect health. It now chiefly affects the foot. About six months ago, he was seized with a difficulty of breathing while in bed, a symptom that has often occurred since, but was soon relieved by sitting up. His want of command over the muscles of the arms and hands is now so great as to prevent him from dressing and undressing.

June 15, 1821.—His left side became affected about six or seven months ago; both hands continue to tremble; he is now unable to turn himself in bed without help; both knees are very weak: all command over the bowels is gone, and he is obliged to take aloes every night to procure a stool. His speech is as distinct as ever. From the beginning, his appetite has never failed, and he has slept well. He assures me he has always been temperate, and his employer confirms his statement, as far as respects the period of his acquaintance with Donahy in this country.

CASE IV.

Elisha Clark, a native of New-Jersey, at the age of forty-eight, perceived his right foot to shake when endeavouring to cross his leg, and about the same time was sensible of a tremulous motion in his right hand when writing. A weak-

ness came on in the whole of his right side, but without pain. A difficulty in articulating words was also evident. Some words (not now recollected) were pronounced with more ease than others. About five years since, he was wholly disqualified from writing, (an employment in which he had been mostly engaged,) owing to the tremulous motion of his hands. Three years ago his left side became affected, and is now (June 1819,) almost as weak as his right side. His tongue, under jaw, and limbs, are in constant motion; his speech is much affected, and he is scarcely intelligible. His appetite has always been good, and his bowels as regular as formerly. He generally sleeps well, but when restless soon becomes composed by lying on his back. The saliva often runs from his mouth. The powers of his mind are unimpaired, and he can still dress and undress himself, except when agitated by any unpleasant occurrence. He sits most easily, when leaning his chair back against a wall; and can walk three miles a day. He is now sixty years old. A blister to his back gave temporary relief. Electricity was tried for several weeks, three times a day, by taking sparks from his right side, without any benefit. Some years will probably elapse before this case finishes its course.

The foregoing cases, I am sensible, cannot be said to advance our knowledge of the healing art, but on the contrary only serve to add to the many proofs of the failure of medicine to relieve the *bodily* afflictions of human nature. They serve to excite melancholy reflections. To see a fellow creature gradually lose all command over his voluntary muscles; and to be obliged to witness the steady but slow progress of that debility, until it affects the heart itself, and life vanishes like an expiring taper, without being able to afford the smallest relief, are sufficient, if any thing were required, to humble our pride as men and as physicians. The cases I have related, may, however, at least prove satisfactory to some of our brethren, by being referred to in the event of their being called to attend a similarly obstinate and intractable case. Besides, we must not permit

the melancholy recitals to pass to the world, without making an effort to speculate on the means of relief.

May it not then be, that the seat of the disease is in the origin of the nerves, and ought not our remedies to be applied to that part, rather than to the distant member, apparently affected from a local cause? This idea appears to have been acted on in the case of Mr. Petty, but the remedies may not have been powerful enough to reach the morbid affection. I should prefer the application of the common caustic to the spine, in the manner successfully applied by Dr. Hartshorne and Dr. Lewis in tetanus, and as I have recommended in hydrophobia,* and I beg leave to suggest its use. The supposition of the affection of the spinal cord in the cases I have related, will be rendered probable by the fact, that in one related by Portal, (*Cours d'Anat. Med.* tom. 4. p. 116,) quoted by Mr. Abernethy, and which agreed in the symptoms with those of Donahy, "that part of the spinal marrow which was included in the cervical vertebræ, was so hard as to have the consistence of cartilage, and the membranes of this portion were very red, as if inflamed." In such an affection, especially in the incipient state, the external inflammation of the surface over the affected part, and along the course of the spine, would seem to promise much, by relieving the internal parts from the effusion, or diverting the diseased action to a less important part.

Mr. Abernethy relates, at the close of his interesting paper, the case of a gentleman who cured himself of an affection which commenced very much like that of Mr. Petty, by a violent walk of five or six miles.—See *Obs. on Dis. of the Spinal Marrow*, *Edinb. Med. and Surg. Journal* for 1818.

* Tie a piece of sponge to a fork or small stick, and after dipping it in a solution of the caustic in water, in the proportion of a drachm to the ounce, apply it along the spine, until a considerable redness is produced. See *Med. Recorder*, vol. ii. p. 351, and vol. iii. p. 170.

ART. XII. *A Case of Epistaxis successfully treated by Immersion.* Communicated in a letter from JAMES KENT PLATT, M. D. Member of the College of Surgeons, London, to JOHN B. BECK, M. D. of New York.

BELIEVING with an elegant writer that "examples are eloquent," I am induced to transmit to you the following instance of the benefit of immersion in epistaxis. It is in physic as in morals, we do not need the inculcation of new precepts so much as we do to be reminded of our duty to practise those which already exist. I am aware there is nothing novel in the *application of cold* in the disease I have mentioned—for its salutary effect in the restraint of hæmorrhage, is as well established as any principle in medicine. My object is to show that it will be found effectual when carried to the extent here employed, though it frequently disappoints our hopes when it is but partially used. I have met with no writer who recommends immersion, and I would not be understood to advocate its unguarded or indiscriminate use.* Many cases require no professional aid: others may be readily imagined, wherein the powers of life might be so far depressed, or the lungs so irritable, as to render the production of rigors hazardous and unwarrantable. Whenever the cold bath is employed, the patient should continue in it a considerable time, as otherwise the reaction would probably be so great as not only to defeat our intention, but actually to change the character of the remedy, and aggravate the very symptoms we wish to relieve.

In a disease like this, when the bleeding has continued until the patient is well nigh exhausted, and every by-stander is filled with alarm and dismay, it is impossible to suggest any practice which will not require the steady exercise of a firm and well disciplined mind—yet, I conceive no

* The late Dr. Thomas Bond, a physician of this city, remarkably distinguished by the originality and boldness of his practice, was in the habit of using the bath, in all hæmorrhages, even those of the lungs.

EDITOR.

greater danger will accrue from the use of cold by immersion in the hands of the unwary and inexperienced, than of any other energetic constitutional remedy. It has been customary in extreme cases of epistaxis to apply compression to the bleeding vessels, by a dossil of lint introduced through the posterior nares, after the method of Le Dran: but setting aside the natural repugnance which most people have to any thing like a surgical operation, and the ordinary inconvenience attendant on the procedure, instances occur in the asthmatic patients, when the irritation of the fauces by ligatures, &c. cause so much difficulty of respiration, as to impede seriously the execution of the plan. In such cases, I should advise immersion, if on trial the common means had failed, unless circumstances existed strongly condemning its use.

Yours truly,

JAMES KENT PLATT.

September 11th, 1820, an extremely warm day, I was called about two o'clock, P. M. to Major J. Walworth, aged thirty-six years, of the sanguine temperament, with a very plethoric habit, and short neck. He had been seized two hours before, after severe exercise, with bleeding at the nose, and it still continued in almost an uninterrupted stream. His pulse was weak, his skin cool, and he felt languid and faint. Venesection had been employed, agaric and other styptics applied, and both nostrils filled with dossils of lint, wet in a solution of alum. These had no other effect than to turn the stream down his throat, to excite irritation in the fauces, and produce expectoration. Ice had been applied to the head, back of the neck, axilla, &c. with no better success.

The patient had now lost, it was estimated, more than four pounds of blood, and it seemed expedient to adopt some energetic mode of treatment. On consultation with Dr. Mooers, it was determined to immerse him in a bath of well water, until an ague should be produced. This effect took place in about a minute after his immersion, and

the rigors continued a few minutes after his removal, to the degree, that he shook as if in the cold stage of a severe intermittent. The happiest success attended the experiment: the hæmorrhage abated before he left the bath, soon ceased entirely, and did not recur.

He was ordered to be kept cool, the ice water was to be continued on his forehead and scrotum, to be cautious not to disturb the coagula in his nostrils, to take repeated doses of nitre, and to observe the strictest abstinence and quiet.

8 P. M. His pulse has risen and skin become warm, he breathes through his mouth only, and being asthmatic from corpulency, his system appears considerably irritated from the obstruction in his nose. The purging salts in common use having, on former occasions, proved offensive to his stomach, and considering it important to prevent vomiting, I ordered him to drink freely of cream of tartar, and to continue the former directions.

Sept. 12th. The laxative has affected him, the febrile symptoms have subsided, and he breathes through one nostril.

13th. The coagula are loosened, he breathes well, has experienced no catarrhal affection from the bath,—is convalescent.

Plattsburgh, (N. York,) Jan. 15, 1821.

ART. XIII. *A Case of Uncommon Disease.* Communicated by
EDWARD COVELL, M. D.

THOMAS O——Y, aged about twelve years, the son of an English gentleman who arrived in this country a short time since, is the subject of the following history. During his voyage, he suffered much from sea-sickness, which was not only violent, but protracted—and his constitution on his arrival, was evidently impaired. He had not entirely recovered from the effects of this illness, when, having imprudently exposed himself to a current of air for a considerable time, he was seized with pain and rigidity of the mus-

cles of the neck. This was deemed a trifling affection, and no applications were immediately made. Its continuance, however, induced his parents to seek my aid, and according with them in opinion, that the complaint was not of a serious character, I satisfied myself with a prescription of vol. lin. to the affected parts, and a cathartic. The occurrence of a violent pain in the head, and of great *soreness* of the scalp shortly after, induced me to order the head to be shaved, and the repeated application of blisters, together with active purges of rhubarb and calomel. By this treatment the symptoms were mitigated—but in a short time the pain of the head assumed a periodical character, occurring every evening about the time of his going to bed. This circumstance led me to administer bark and laudanum, from which I confidently expected my patient would derive relief: but the remedy did not answer my expectations, for although the violence of the pain was diminished, it continued to recur,—and to the symptoms already detailed, were superadded great torpor of the bowels, and a weakness of the lower extremities, not yet amounting to paralysis.

Under these circumstances, eight drops of Fowler's solution were prescribed three times daily, the use of which was continued nearly a week. At the end of this period, the pain in the head was almost entirely relieved, but the weakness of both the upper and lower limbs had alarmingly increased, and he was *now* nearly incapable of all motion. The alimentary canal continued inactive, and no evacuations could be procured without the aid of cathartics.

After having tried a variety of stimulants, such as the vol. tinct. of guiac. and finally the application of electricity, without any benefit to my patient, I was led to an impression that his disease was of *gastric* origin, and the following mode of treatment was entered on and pursued with great assiduity. Every third evening he was directed to take a cathartic of ten grains of rhubarb and three grains of calomel, the operation of which it was always necessary to assist by an infusion of senna and gentian. In the interval of the exhibition of the purges, he was ordered ten grains of

the carb. ferri, in combination with ginger, thrice a day—and twice daily, he suffered long continued frictions to his limbs with the flesh-brush. He was also directed to the use of the stimulating condiments, which he took in considerable quantities. For a short time, this plan promised to be beneficial. The cathartic medicines produced dark coloured and highly fœtid stools, and the friends of the patient concurred with me in believing that the limbs were acquiring some strength. At the end of six weeks, however, no material alteration was, on the whole, to be perceived, and I ceased my visits, barely directing that an evacuation should be obtained daily from the bowels, either by stimulating enemata, or by an infusion of senna.

Feeling a deep interest in the fate of my patient, I communicated an account of his case to a physician of distinction and extensive practice. He advised that a pair of large caustic issues should be established on each side of the spine—that he should daily use a tepid salt-bath; and recommended, though in doubtful terms, a salivation.

Shortly after the receipt of this advice, and as I was about entering upon the practice which it recommended, my patient was seized, after a trifling exposure, with an affection of the breast, attended by most harassing efforts to cough, and an ardent fever. His pulses were very frequent and somewhat tense. His expectoration was exceedingly difficult—for the muscles concerned in this operation, seemed to partake of the paralysis of the limbs, and I had serious apprehensions that death would occur from an accumulation of mucus in the *brochiæ*, &c. Venesection appeared a very questionable remedy, and yet the degree of fever seemed to require it. I however omitted it, and contented myself with the application of a large emp. epis. to the breast. To free the lungs from the load with which they were oppressed, I attempted, by the exhibition of four grains of tart. ant. and twelve grains of ipecac. to procure emesis—but in this I failed, from the inirritability of the stomach. Antimonial medicines in large and frequent doses were, however, employed in combination with a strong decoction of the

polyg. sen. and by these remedies in the course of a week, I had the satisfaction of seeing my patient convalescent—and this satisfaction was greatly heightened by observing that the disease also with which I had so long and so fruitlessly contended, had, in a great degree, yielded to the *morbid excitement* with which the system had lately been struggling: and I am gratified in being able to record, that the cure of the paralysis thus commenced, has since been completed without the adventitious aid of any article of the *materia medica*.

Wilkesbarre, Feb. 22, 1821.

ART. XIV. *A Case of Quartan Ague, cured by repeated Emetics.*

Communicated by ALLEN M'LANE, M. D.

ABOUT the first of December, 1820, I was requested to visit Mr. M., a gentleman from the eastern shore of Maryland, with intermittent fever.

I found, upon inquiry of the patient, that about seven weeks before he had been attacked with a tertian fever, but that for the last five weeks he had been labouring under the disease of the form with which he was then afflicted, which was a quartan.

He informed me that the district of country in which he resided had been, and then was, very sickly—much more so than usual, and the diseases uncommonly fatal. They varied from the highest grades of bilious ever to the mildest form of an intermittent—and not unfrequently terminating in typhus. He was induced, from the above circumstances, to call in his family physician early in his disease, and had by his advice taken an emetic, been repeatedly purged with calomel and other medicines, and had used the bark in large quantities, and under various forms, without the smallest advantage. He had been advised a change of air—and in pursuance of this advice he had come to this town.

His constitution appeared not a little impaired by the continuance and violence of the disease—and the first fit in which I saw him, did not at all lessen my alarm for his si-

tuation. His stomach was very irritable, and would bear medicines of any kind in but small quantities, and at long intervals.

After cleansing the stomach and bowels, as I supposed, sufficiently, by active purges, I tried the bark in a variety of forms, without the least advantage. I then used Fowler's mineral solution, sulphur, blisters, mercury to produce ptyalism, spirits of turpentine, and lastly, the sulphat of copper, so highly recommended in this particular form of intermittents, by the most eminent of the faculty. The latter medicine was used according to the following formula:

R Sulphat of copper, grs. 5,
Opium, pulv. - 5,
Castile soap, q. s.

to be mixed intimately, and divided into 20 pills, four to be taken at equal intervals during the day.

I may here observe, that I did not at any time find the pulse sufficiently active to call for the loss of blood.

The patient remained under my care till about the first of February, 1821, at which time he went to Philadelphia—having missed, I think, but two chills during the period of two months.

As his disease still continued, he applied to Dr. Physick; and by the advice of this most excellent physician and surgeon, took the sulphat of copper and several other approved medicines, without arresting his complaint.

After remaining in the city about six weeks, he returned to this place, having almost made up his mind to permit the disease to take its course. But I prevailed upon him to make another trial, and I confess, I considered it the last.

My friend Dr. James Tilton, jr. having informed that he had found, whilst living in Maryland, the most decided advantage from repeated emetics, in the most obstinate cases of intermittent fever—having myself been in many instances well convinced of the wonderful power which they exert on the human system, by changing its diseased actions, and recollecting the forcible observation of the late illustrious Dr. Benjamin Rush, that he had never known an intermittent fever refuse to yield to the bark, if sufficient de-

pletion had been used, and the stomach properly cleansed—I determined to exhibit an emetic every day, for three or four days in succession.

On the morning of the day immediately succeeding that upon which he had a most violent chill at ten o'clock, I dissolved six grains of the tartarised antimony in one gill of warm water, and gave one table spoonful every five minutes until it vomited freely, discharged a very large quantity of bile and mucus, and produced great prostration of strength. He slept for several hours after its operation, and was permitted to take a small quantity of light nourishment in the afternoon. A mild anodyne was given at bed-time.

The following day the same course was pursued, and with similar effects, except that the debility was evidently greater.

As I was anxious to cleanse the stomach completely, on the third day I administered the emetic in the same manner as on the two preceding days, but after giving several doses of the medicine, severe retching was produced, without any discharge of bile or mucus. I discontinued it, and gave him about 20 drops of laudanum. He rested well for several hours, and when he awoke, as he was very weak and entirely free from fever, I gave him the following preparation of bark:

R Peruvian Bark,	1 ounce,
Cream of tartar,	2 drachms,
Cloves,	2 drachms,
Port Wine,	1 quart,

to be boiled slowly down to one pint and an half—then filtered, and a wine glass full to be taken every two hours. He was soon enabled to take the bark in substance, which his stomach bore tolerably well.

He had no regular fit afterwards while he remained in Wilmington, which was about six weeks, and at present enjoys good health.*

Wilmington, (Del.) July 2d, 1821.

* The use of emetics, exactly as in the above case, is an old Virginia practice, the efficacy of which I have constantly inculcated in my Lectures. EDITOR.

ART. XV. *Case of False Joint united by Seton.* Communicated by NICHOLAS WORTHINGTON, M. D. in a letter to Professor Gibson.

DEAR SIR,

THE material circumstances of the case of false joint, in which I used the seton, are distinctly in my memory—though I have not been able to find the notes, which were taken at the time.

In February, 1820, I was requested to do something for a negro girl about ten years of age, who, eighteen months before, had her arm broken by a horse and cart passing violently over her, and the fracture had never united.

I discovered that the humerus had been broken obliquely in the middle, from within, outwards and downwards. The motion of the parts was similar to that of a flail. The extremities of the fractured portions were rounded, and no attachment between them could be felt, except at the point of a spicula of bone, which had been torn up from the side of the inferior fractured portion of the humerus, and which projected more than an inch from the exterior of the lower extremity of the superior part. This had some little attachment, but no definite connexion with the bone below.

The spicula being superfluous, I made a longitudinal incision down upon it—separated its point from its attachment, where some effort had been made to provide a capsule and a glairy fluid,—and took it off as close to the body of the bone, as the saw could be applied. The incision was closed with adhesive plaster: it united at first, but opened again in the progress of the case, and finally healed without much trouble.

In order to place the seton accurately between the fractured extremities of the bone, the arm was greatly extended, and a seton needle, made for the purpose, to which was attached a skein of silk, was passed through the diameter of the arm, from before backwards. Loose dressings were applied, and the arm put into a sling. In three weeks a mild inflam-

mation, and very slight inconvenience, were experienced by the patient. The arm then began to be more sore, and the discharge to be occasionally discoloured. At the end of five weeks, the soreness continued, but the girl thought that the arm felt stronger—and we believed that it did not yield so easily to flexure as it had done. Nothing occurred for sometime longer, to make us doubtful of success. The girl, however, became feverish—the soreness in the parts exquisite, whenever the seton was drawn, and a thin, bloody, and offensive discharge, ensued. The only conjecture which seemed in any way to account for this sudden change, was, that by the continuance of the seton, the irritation had transcended healthy inflammation, and that the granulations had lost power, and were in danger of not being able to hold their own.

I hoped, upon the seton being removed, that by keeping the parts at perfect rest, and in strict apposition, something yet might be gained. After anxiously waiting five or six weeks more, the arm gradually gained strength, the ulcers healed, and union between the broken parts took place. Although this union is not that of firm bone, yet it has consistency and strength enough to make it a very useful arm.

There still remains much deformity in the limb, but it has sufficient power to carry a bucket of water—to bear about a heavy child,—and the girl is pleased to do many feats evincive of the satisfaction she has in the restoration of her arm.

If the facts stated above be of value, you will make use of them as you suggested to me, and the form in which it is best to present them, I willingly leave to yourself.

Georgetown, D. C. July 2d, 1821.

ART. XVI. *Case of Swelled Leg, occurring in a male.* Communicated by HENRY BOND, M. D. in a letter to the Editor.

SIR,

I SEND you the following case to be inserted in your Journal, if you shall think it merits publication—not that there is any thing extraordinary in the injury sustained by the right limb, or its cure—but on account of that affection of the left limb, which, in every symptom, had so strong a likeness to the *phlegmasia dolens* of puerperal women. If the case possess no practical importance, it certainly does some novelty, as far as I can learn—and possibly some one may hereafter consider it an important datum, in reference to the proximate cause of that disease. The case is taken almost verbatim from the hasty minutes of my note-book—and it is now so long since it occurred, that I will not attempt to supply its imperfections from recollection.

April 15th, 1818. I was called to E. Philips, aged about thirty-two years, a hardy muscular man, master of one of the canal boats that ply on the Middlesex Canal and Merrimack river, between Concord, N. H. and Boston. In attempting to turn over a boat, the workmen found it pressed so heavily on them, that they gave way, and all, except Philips, cleared themselves. His position was such, that he could not escape. The edge of the boat first struck the upper part of the right thigh, directly over the *tensor vaginæ femoris*, grazed down the limb, till it came to the head of the bone, when it threw him down on his left side, with his right knee and leg, and left foot, under the boat.

The internal lateral ligament of the right knee was ruptured—the thigh bone was fractured immediately above the condyles, about two inches above the extremity: the tibia was fractured four inches above the ankle, and the foot was so bruised, that it was in a short time very considerably swollen. There was no deformity or displacement of the fragments of the tibia—and the evidence I had at first of its being fractured, were a slight bruise in the part, and pain

which was exasperated by attempts to turn the foot, or to produce extension. I thought I could perceive some motion in the bone, but as nothing was to be gained by a greater certainty of the fact, I did not think it expedient to gratify my curiosity at the expense of the patient's sufferings. There was a slight bruise on the inner and anterior part of the left knee. The thigh bone was dressed with Dessault's apparatus, as modified by Drs. Physick and Hutchinson. On account of the condition of the lateral ligament, and of the tibia, as little extension was used as possible—that little, however, prevented any contraction or deformity of the limb. The limb, and particularly the knee, became much swollen and very sore. He was troubled with pain and soreness in his back and abdomen, which were much relieved by friction, with vinegar and muriate of soda. It was necessary to make a constant use of laxatives, and he was sometimes severely afflicted with colic. He continued as easy as could be reasonably expected, from the nature of the injury, until the 29th of April. In the afternoon of that day, which was precisely two weeks from the time of receiving the injury, a pain commenced in the lower part of the abdomen, and the groin of the left side, which was very intense during that night and the next day, in the evening of which I arrived. The fractured limb at this time gave him very little uneasiness: it was so well, that I had not seen him for two days. The pain began in the course of the glands and lymphatics, from the top of the thigh towards the *receptaculum chyli*, but there was some pain diffused through the left half of the pelvis. From the groin the pain had gradually extended down the thigh to the knee, and into the calf of the leg, where it had reached when I arrived, and where there was, then, the most excruciating pain. It was a little more than twenty-four hours in passing down thus far. The limb was one third larger than natural, and the swelling was greatest in the thigh and knee.

May 31st, morning. The leg is more tumid than any other part of the limb—skin is pale, hot, glossy, and has a edematous appearance—so extremely sore, that he can hardly

endure to have it touched or moved—white coat in the middle of the tongue—considerable arterial excitement—spirits depressed, and appetite gone. Swelling has extended into the foot. Says he has suffered more in this limb the last thirty-six hours, than the whole amount of suffering in the fractured limb.

5th and 6th. Pain is principally in the groin, hip, and lower part of the back—pain in the limb abated, but swelling continues—swelling and soreness about the hip and back, similar to that at first in the limb.

8th. There is very little pain or soreness in the left leg, but the swelling has abated very little. The fractured thigh bone gives him no inconvenience—seems to have united. The swelling has gone down in the whole limb—and the fluctuation, which was very distinct in the knee-joint for several days, has nearly or quite left it. The principal difficulty in the right limb is now in the knee-joint. The internal lateral ligament being partly, if not wholly ruptured, there is reason to apprehend, that this joint will remain weak and unfit for use for a long time.

10th. The swelling in the left hip and thigh has subsided more within twenty-four hours, than it has before since the attack. The swelling began to abate in the top of the limb, and the abatement is extending gradually down to the foot. The soreness about the ruptured ligament of the right knee is abating—appetite good.

12th. The thigh and knee of the left side are reduced to their natural size, except some swelling and stiffness in the ham, which has an irregular ridgy feeling. The calf of the leg, the ankle and foot, are still swollen: in the foot the abatement of the swelling is very trifling. As fast as the swelling abates, the limb acquires its natural feeling, and some strength. Appetite and spirits good.

16th. The knee-joint of the fractured limb has gained strength fast within a few days, and the injured ligament has but little soreness or swelling. A callus indicates very clearly where the tibia was broken, since the general tumefaction has subsided. To-day he sat on the side of the bed,

and bent the knee of the fractured limb as much as six inches, or about eighteen or twenty degrees. At the last visit, he could not bend it more than six degrees.

The foot, ankle, and lower part of the leg of the left side remain sore and swollen—less, however, than for a short time after they were taken. June 15th, found him walking on his crutches, as he has done since the first of the month. He is unable to make use of the fractured limb, the knee continuing weak, but gaining strength. The joint has gained nothing in pliancy of late. He can bend the knee two-fifths of the way to a right angle—there is some stiffness in the ankle. The left leg, particularly the lower part of it, remains considerably swollen. I have not seen the patient since, but was informed by his brother, a few months after, that he could walk without crutches, and was able to work.

I have omitted the particulars of the treatment, as it would have extended this paper too much. The principal points of my practice were as follows. When I first saw the patient after the attack of the swelled leg, I prescribed a hot fomentation of a strong decoction of wormwood, with one-fourth part of proof spirit. This application was frequently repeated, to the leg, knee, and thigh, for the first day or two, and with the most unequivocal advantage: nothing else was so efficient in allaying the excruciating pain. Nitrate of potash was added to the fomentation, and vinegar sometimes substituted for spirit. In the subsequent treatment, I directed friction to be employed, with vol. liniment and spirit of turpentine combined, with opodeldoc, with the lin. hyd. comp. This liniment was employed about the groins, as the glands were so swollen as to form a considerable ridge through it. The tincture of opium, camphor and brandy, were employed, when there was much pain. I could not form a decided opinion of the comparative value of these applications. Either of them applied with friction, was beneficial. I employed anodynes internally, in the form of Dover's powder—or a combination of opium, ext. of stramonium, camphor and tartarized antimo-

ny in the form of pills. The first passages were kept free by castor oil, or the compound powder of jalap. I used antimonials to allay the febrile symptoms. Bloodletting I did not employ, as I probably should, in another similar case.

ART. XVII. *Desultory Remarks on several Cases of Disease.*
Communicated in a Letter from DANIEL WILSON, M. D. to
Richard Povall, M. D.

DEAR SIR,

YOU have no doubt read in the first number of the Philadelphia Journal of Medical and Physical Sciences, the case of tape worm, reported by Professor Caldwell, of the Transylvania University. The limited time, however, that he necessarily devoted to the case, being only on a visit, has led him into some errors, which I beg leave to correct. Although not the prescribing physician, I was, and am, in the almost daily habit of seeing and conversing with the gentleman who unfortunately furnishes the case, and had therefore an opportunity of studying it in detail.

I shall also offer an hypothesis different from Dr. Caldwell's, as to the manner in which the amalgam is confined in the intestines. This hypothesis was formed before Dr. Caldwell became acquainted with the case, and although I respect the ingenious hypothesis he has advanced, I am not prepared to surrender my own. He states, that by persevering in the remedies prescribed at different times, thirty feet of the worm had been discharged in three years, whereas, at least three hundred feet came away in that time.

It was within an hour after taking the sixteenth dose of the amalgam, that the cathartic was exhibited, and not the next day, as stated by Dr. Caldwell.

It was not until the patient had returned from his trip to New-Orleans, which was in June, that he took the last $\frac{3}{4}$ iv of the amalgam—and this was done without medical advice.

At this time he first referred the load to his intestines—upon which, I placed him in an horizontal position with his abdomen downwards, and ascertained the existence of the ball or mass so well described by Dr. Caldwell.—He had before this referred the load to his stomach, whenever he mentioned it to me, or to the prescribing physician, and I believe he led us into an error, by his want of knowledge of the parts of which he spoke. The novelty of the case induced the prescribing physician to suppose, that the load at the stomach was possibly the result of a false sensation, caused in some way by the great quantity of amalgam taken, together with the great quantity of worm discharged. He supposed too, that the amalgam came away without arresting the notice of the patient. I concurred in this supposition. The manner in which Dr. Caldwell notices the remedy, is calculated, I think, to impair its standing: but in a similar case, I should be inclined to use the same remedy with great confidence—if, what too generally happens, other remedies fail. I would extend the intervals, however, according to Dr. Darwin, to two hours, or even longer. In the case before us, I suspect the unfortunate effect was produced by repeating the doses in such quick succession, as not to allow to the intestines time to relieve themselves of one before another was taken.

Dr. Caldwell thinks it most probable, that this “metallic ball” is lodged in one of the cells of the ascending colon, and that it is enveloped in a sac by the production of a new membrane. To complete this hypothesis, I am persuaded it will be conceded, that this sac must be divided into two portions, one of which was formed between January, when he took the first $\frac{3}{4}$ vi of the amalgam, and June, when he took the last $\frac{3}{4}$ iv, and the other after the latter month. I entertain too high an opinion of the talents of Dr. Caldwell to suffer myself to treat his opinions lightly—yet, as a disciple of truth, living in an age of inquiry, he will not consider me as taking up the subject merely to differ with him, but to elicit the true situation of the patient. The case being novel, I had to rely on such medical authorities as I could

command, to enable me to form an hypothesis of its nature ; and finally I thought it was most probably a case of introsusception. The idea of obstinate costiveness is however so strongly associated with introsusception, as to present a strong objection to my conjecture : yet if we consult medical authorities, we shall find the disease mentioned, as having been detected in infants, where no costiveness was connected with it, nor indeed any other symptom indicating such a condition. We have then only to admit, that introsusception may exist in adults as well as infants, without being necessarily associated with costiveness, and my hypothesis will be rendered quite probable.

The gentleman who is the subject of the case, has lately been distressed with a violent pulsation in the affected part, which he says was sensibly felt on applying his hand,—that it continued for nearly twenty-four hours, and that he has since occasionally felt the same sensation for a short time.

I have an additional reason for noticing this case—and although it is last, it is not least in point of importance to me. The gentleman thus afflicted is my friend, and I am anxious to ascertain how far it is possible to relieve him, and I solicit your own and the opinions of every member of the faculty, who will favour me with a line on the occasion.*

* Not long after the publication of this interesting case, we received from an anonymous source, the following communication. The suggestion which it contains seems to us to be deserving of attention. On the case being submitted to Dr Physick, he at once suggested the same remedy, and in anticipation of our correspondent. To this he was led, independently of any general reasonings, by the fact of a man having been relieved of a portion of tin bougie, which in some rude trials had broken, and was retained in the bladder, by injections of crude mercury into that viscus.

EDITOR.

“ The patient’s stomach and bowels being in the most tranquil state, and when he could submit to all proper discipline, a quantity of pure quicksilver (one ounce, every twenty-four hours, more or less) should be given, and its discharge or retention carefully observed. As the mercury is thrown out, it ought to be carefully weighed, to learn what proportion is retained, and examined to see if it brings away with it any tin. Being retained, in part, the tumour would probably furnish, by its outward feel, proof of a consistence so softened, as to admit of its being moulded by the fingers into a shape more favourable for its expulsion.

On the 24th of January, 1821, I was called to visit a gentleman affected with that form of acute gout denominated regular. He is an Englishman, 42 or 43 years old, stout, corpulent, athletic, and plethoric, and engaged in mercantile pursuits. He has suffered repeated attacks of the same disease, and assures me that the present one is marked with more than usual violence in its symptoms, as well as rapidity in its approach. Both feet were tumefied, inflamed, and painful to a considerable extent. His pulse was 100, but neither full nor strong. The head and stomach were much affected. On surveying all the symptoms, although not forbidding the use of the lancet, they did not strongly urge its use, I therefore held it in reserve, determined to apply it, if the symptoms either increased or did not yield to other remedies. He had his feet abundantly protected against the cold, which was then very great, and I ordered this precaution to be continued. For five days in succession I gave him a cathartic, composed each time of sal Epsom. one ounce, and carbonate of magnesia, two

“Should the quicksilver continue to be evacuated without any tin in union, it would lead us to fear that some intervening substance had prevented its coming in contact with the mass. But tin being found united with the quicksilver, we should be encouraged to hope that the whole mass might ultimately thus be brought away.

“In case of the first doses being wholly retained, it ought to prompt us to persist; and if no untoward circumstances occur to forbid its continuance, we might flatter ourselves that it had united with the old amalgam, and would ultimately liquefy the whole mass. This fortunately happening, I would next recommend as large potions of the blandest olive oil as the stomach would bear, to be repeatedly and regularly taken; avoiding, as much as possible, every source of irritation to the intestines, for fear of producing spasm.

“For, it appears to me, from Dr. Caldwell's own account, that the small quantity of amalgam which the patient has voided, may have been nearly all pure mercury, squeezed off by the action of the bowels and abdominal muscles, thus leaving the remaining portion too hard and solid to pass. Crude mercury has not, I believe, generally been found injurious, when given even in large quantities, some few opinions to the contrary notwithstanding; and it certainly seems as yet, to have been inactive, in this gentleman's case, though so long retained.

“I confess I am doubtful whether there is not some danger, in giving quicksilver, of some small globules falling by the epiglottis into the trachea; should these fears be thought not groundless, it would be easy, with a flexible tube, to pass the mercury below the entrance into the windpipe.”

drachms, to be rubbed up together, and taken in a half-pint of water. The consequence was, a rapid and regular abatement of every symptom. Of the first two or three doses, each procured but three or four evacuations: they were, however, copious, dark, and offensive. On the sixth day I suspended the use of medicine, but on the seventh, as a matter of prudence more than from any return of the symptoms, I repeated the dose, and on the eighth day, discharged him as convalescent. To restore the tone of his stomach in particular, and of his system generally, I directed him to take an infusion of the quassia amara with the acid. vit. arom.

In his diet he was restricted to rice water, and cornmeal gruel, for the two first days—but the prompt relief derived from the medicine, induced me to indulge him, at his own request, after that, in the moderate use of weak tea without milk.

By reviewing the history of gout, we shall find it is only a few years since the most eminent medical men advised, in such cases, to look on, and, if possible, retain the disease in the extremities, as the safest course to be pursued.

It is to Professor Chapman I am indebted for the simple but effectual plan by which I cured my patient. I had the pleasure of hearing several of his lectures in February, 1817; but when he dwelt on the gout, I derived impressions unusually interesting. Added to this, I have lately procured and read a copy of his work on Therapeutics and Materia Medica. Dr. Chapman recommends a combination of rhubarb and magnesia—but for the last two years I have so often witnessed the valuable effects of the combined use of salts and magnesia, that I determined to give the preference in this case to my own hobby. After an experience of nearly eighteen years, reared on the medical honours of the University of Pennsylvania, I believe that Epsom salts and magnesia combined, furnish one of the most effectual cathartics in bilious fever, the

celebrity of jalap and calomel notwithstanding.* An ounce of the first added to two drachms of the latter make a bulky mass, but I have very frequently given it. I, however, often divide this mass into four papers, and direct one every hour or two until my views are accomplished. I have found it, generally, to evacuate the bile as copiously as jalap and calomel, without being succeeded by that debility which so often is the consequence of these articles, not to name another objection, quite as strong, which applies to calomel particularly.

* This combination of magnesia and Epsom salts is much used in the practice of this city, and is deserving of all the praise bestowed upon it. We find, however, that a drachm or two of each of the ingredients quite active enough as an evacuant.

EDITOR.

ART. XVIII. *Observations on several Cases of Disease, &c. &c.*
By S. COLHOUN, M. D.

On the Thyroid Gland.

THE attempts to discover the ducts of the thyroid gland have been various and unsuccessful. From its parenchymatous structure, its situation below muscles which press upon it in every act of deglutition, from the fluid frequently found in its substance, the large supply of blood conveyed to it, and its proximity to the trachea, larynx, and œsophagus, it has been supposed to secrete a fluid; and the internal surface of these passages has been examined with great attention, to discover the openings of its ducts. By Morgagni, the parts between the gland and the adjacent cartilages were scrutinized in vain—the gland itself was pressed, the larynx, the trachea, the pharynx, and the œsophagus being at the same time opened, first on one side and then on the other, sometimes from behind in the direction of their length, and then drawn asunder, in order to ascertain whether any fluid was secreted by the gland, and the precise place of its discharge—but without success. With

the same view, the surface of the trachea was examined in the anterior angle of the glottis—a situation in which Santorini had described the opening of a small canal, in which a bristle was passed: an opening, penetrating for a short distance, was discovered by Morgagni, in one instance, and the pursuit was relinquished.*

In the year 1741, Bordeu examined a subject, in which he found two small openings, penetrating the first cartilaginous ring of the trachea,† and called the attention of the medical public again to the ducts of the thyroid gland.

In another subject, he found the first cartilaginous ring penetrated in the same manner by openings, and its lining membrane exhibiting small apertures, which were perceived with difficulty—and when bristles were introduced into them, they were found to terminate in the holes in the cartilages: and, when pushed farther, lost themselves in the gland, inducing the supposition that they were the passages of its ducts, opening into the trachea. These apertures, however, are not peculiar to the part of the trachea over the openings of the cartilages, but are distributed over the whole surface;‡ a sufficient proof that they have no particular connexion with the thyroid gland or its ducts.

Dr. Gibson, Professor of Surgery in the University of Pennsylvania, was induced, by the remarks of Bordeu, to examine this subject. He inserted the pipes of a mercurial injecting apparatus, directly into the small apertures in the first cartilaginous ring of the trachea, and found that the mercury passed with ease through the small canals, but met with resistance when it reached the thyroid gland, and in three or four instances, it penetrated the cellular membrane surrounding it in the substance of the gland, “but in all probability, the mercury passed in each case from a rupture of the cellular tissue, and did not follow the natural course of the duct.” The cellular tissue was also filled more completely from the ventricles of Galen, by the same apparatus.

* Morgagni *Oper. Omn. Epist. Anatom.* ix. art. 34.

† Chapman's *Journal*, &c. vol. i. p. 56.

‡ Wistar's *Anatomy*, vol. ii. p. 63. 64. *Philad.* 1814.

To this we would beg leave to observe, that the direct introduction of the small steel pipes into the holes of the first cartilaginous ring of the trachea, would certainly rupture and destroy the ducts, if they existed there, and render the experiment insufficient to prove their existence. In the injection of mercury from the ventricles of Galen, the pipes must have been inserted through the skin, and the mercury have found its way into the cellular membrane, and of course have no connexion with the duct.

As far then as the subject has yet been investigated, the existence of these ducts appears to be without proof. Bordeu mentions that the internal surface of the first cartilaginous ring of the trachea, is covered with them, and that bristles passed into five of these openings, terminated directly in the apertures, and were lost in the gland: the hard substance and fineness of the point of the bristle, as well as the loose texture of the membrane, lead to the suspicion, that the hand of the operator gave them that direction, and that there exist no ducts in this situation; a circumstance rendered highly probable by further observations, which show that the small holes in the cartilage in the front of the trachea, are intended for the transmission of arteries and veins.

In one subject, a woman, the aorta was injected with a composition of red and white lead, made into a soft paste, with linseed oil, to which was added turpentine varnish in sufficient quantity to make it of a proper degree of fluidity. This mixture soon hardens, and presents the vessels nearly in their natural state. Arteries were found to penetrate and pass through the openings, and were distributed on the lining membrane of the trachea. This observation was repeatedly confirmed, but as the arteries did not occupy the whole space of the small apertures, it was suspected that they also afforded a passage for veins. Accordingly, the veins on the front of the trachea were injected with mercury, and the fluid mixed with venous blood, could be distinctly traced through the openings by the side of the injected artery, and was seen penetrating on the inside of the trachea;

rendering it probable that the ducts of the thyroid gland, as represented by Bordeu, have not yet been demonstrated.

Foderé mentions, that if the lower extremity of the larynx, well washed with a weak solution of potash and dried, be completely stopped, and a tube with a bladder filled with air attached to its one end, be accurately adapted to the opening of the glottis, by compressing the bladder, the thyroid gland is increased in volume—"the experiment succeeded, though in a slight degree, with alcohol: on cutting the gland after compressing the bladder, the odour of that fluid is distinctly perceived."* As potash dissolves both dead and living matter, it is to be regretted that Mr. Foderé did not signify the quantity of potash in the solution, as the extremely delicate membranes, lining the mucous follicles, may have been destroyed, and a passage formed for the air into the cellular membrane around the vessels, which penetrate the thyroid cartilage, and thus gain admission to the gland and distend it: and with regard to the odour of the alcohol, which was perceived on cutting into the thyroid gland, when the trachea had been injected with it, the proximity of the trachea of course renders it uncertain, whether the odour did not arise from the trachea itself, and not from the interior of the gland. As relates to the swelling of the gland produced by lifting great weights, and during labour, as the rupture of blood-vessels may be supposed as the cause of its distention, of course, no connexion by direct apertures, between the trachea and the thyroid gland, can from this fact be demonstrated.

Cases of Erysipelas.

It is stated by Wiseman, that a cerate made of olive oil and white wax, cures erysipelas.† Desault, and late writers, generally reprobate the practice.‡ Drs. Dean and Little, recommend the mercurial ointment.§ Lard is equally beneficial, and more safe, as the following cases show.

* See Vol. I. p. 57 of this Journal.

† Eight Chirurgical Treatises, p. 60, 64, et seq. Ed. 1719, Lond.

‡ See Desault's Journal, quoted by Darwin, Zoon. vol. ii. p. 391. Bost. 1803.

§ See Medical Recorder, Philad., and Chapman's Journal, No. 3.

A young woman was taken with fever, a burning sensation, redness and tumefaction of the skin, of the eyelids, and face, attended with headache and inability to sleep: the fever increasing, as well as the local symptoms—fresh lard was applied to the face, and the pain soon relieved. It was continued during the night and next morning; and the pain and swelling were much abated. Immediately on its first application, she experienced ease, and in three days was perfectly well. The danger attending this inflammation in young people, renders this fact interesting, particularly as it is liable to extend in infants, and under the usual treatment, often proves intractable.

A man, aged about seventy years, was attacked with an erysipelatous inflammation of the upper part of the foot, with pain so great, as to prevent his walking but with difficulty. The lard was applied on the morning of the second day, with immediate relief: he continued easy, and slept well during the following night—the inflammation gradually abated till the following day, when he was nearly well. In two days afterwards, he had a relapse, with considerable increase of the inflammation: on re-applying the lard, he felt immediate ease, and in the space of two days, was perfectly cured. This subject was disposed to dropsy, and had been cured of that disease some months before.

Another case occurred in the country, under the care of Dr. Culbertson, an intelligent practitioner, in which the cure was prompt and decided. The danger of producing salivation in some constitutions, by mercurial ointment, and the speedy relief which this remedy produces, renders it highly valuable. In a case of erysipelas of the face, attended with great itching, sweet oil applied to the surface, softened the skin, and abated the inflammation, and it is probable, will answer in the treatment of this disease. These facts prove clearly, that the position so generally held, that oily applications are hurtful in erysipelas, is not true. In the mercurial ointment, the mercury has been supposed to be the active agent:

it is probable, from the above facts, that it owes its virtues to the lard alone.*

Cases of Exostosis.

Exostosis occurs very much among the poor, from the effect of rheumatism and syphilis: it becomes indolent, and frequently resists the use of all remedies, both general and local. After the constitutional disease which produced it has been removed, it remains stationary, impeding the motions of the limb.

On dissecting a tumour of this kind, blood-vessels of an increased size were found ramifying over its surface; they generally spread from two or three trunks over the tumour—an observation which suggested the plan of treatment detailed in the following cases.

J. S. had an exostosis of a flattened form, apparently equal in solid contents to a pullet's egg, and situated on the fore and upper part of the tibia. It had continued stationary for eighteen months: the pain of the tumour had nearly subsided, and was in an indolent state—he had taken a great variety of remedies, till within a month before he applied to me. On examination, several arteries, as large as a crow-quill, were discovered running over the edge of the tibia, with a distinct pulsation. At first it was attempted to obliterate them by compresses, but from the pain and insufficiency of this plan, it was relinquished. It was then determined to divide the arteries. The skin was first marked with ink, immediately above them, and they were then divided. They bled freely: lint was inserted into the scarifications, and covered with a plaster of simple cerate. On the next day the lint was removed, and the wound gradually healed. In about five days the tumour had sensibly diminished: the pain, as soon as the irritation produced by the division of the arteries had abated, also declined; and in about three weeks the tumour had lessened considerably. He was able to continue his ordinary employment, and in

* We think differently, since other mercurials, as the *watery* solution of corrosive sublimate, and salomel *dusted* on the part, are serviceable. ED.

about two months after this period it was entirely removed.

Two other cases have occurred since that time, in one of which a few of the arteries were divided: the abatement of the tumour was gradual, and the patient finally recovered. As, however, in this case, shortly after the operation, other remedies were used by the consulting practitioner, and the operation was partial, no positive decision can be drawn with regard to it. In the other instance, the effect of the practice was decided: the tumour was about the size of a walnut, and was seated near the middle of the tibia. It had been in an indolent state for nearly six months, and was slightly painful on pressure. A small artery crossed it obliquely from above downwards, and could be felt distinctly pulsating. It was divided and dressed as in the first instance. In the course of eight days the tumour had diminished, its sensibility gradually lessened, and in about a month, the patient was perfectly well.

Mr. Astley Cooper has revived the old practice of denuding the bone, as appears from a paper on this subject by Professor Gibson, in the last number of this Journal. The division of the arteries of the part, will probably prove an equally effectual and certainly a less painful operation. In some instances, an incision round the whole circumference of the tumour, dividing the periosteum to the bone, will be a useful substitute for scraping its surface, particularly when the tumour is extensive, and the arteries are numerous, or difficult to be discovered. May not the same plan be used to cure old ulcers? On examination after death, the arteries supplying a sore, attended with a caries of the fibula, were found to be enlarged. By dividing them, irritation would probably be diminished or entirely destroyed—whereas, so long as the blood-vessels continue of a greater size, it is probable that the various stimulating applications which are used, will often only increase the irritation, and retard the cure.

REVIEW.

ART. XIX. *Report on the Epidemic Cholera Morbus, as it visited the Territories subject to the Presidency of Bengal, in the years 1817, 1818, and 1819. Drawn up by order of the Government, under the superintendence of the Medical Board.* By JAMES JAMESON, Assistant Surgeon, and Secretary to the Board. Calcutta, 1820. pp. 328.

WE have here an account of one of the most singular epidemics known to have ever wasted the human race, and which is equally interesting from its novelty, and the awful extent of its devastations. The work bears the highest marks of authority, professing to be compiled from the reports of all the members of the medical department of this portion of the East India Company's government, who could be induced by a circular letter to furnish the results of their observations. The British nation, from the vast extent of their military system, possess immense advantages for this mode of inquiry, in the facility of reference to their medical officers, and such opportunities have been, on many occasions, most usefully improved. In the present instance, it appears to have afforded one of the completest accounts of an epidemic, as far as the field of inquiry extended, with which we are acquainted.

The strong hold of the British possessions in India, consists, as is well known, in the provinces under their government in Hindostan and the Deccan. Of these, the power of the presidency of Bengal extends, directly or indirectly, over the greater part of Hindostan Proper, or that portion lying north of the river Neerbudda, and the twenty-first degree of north latitude. It embraces, in short, nearly the whole valley of the Ganges, and the mouth of the Burrampooter river—a region, in its circumstances of heat and

variety of climate, liability to periodical inundation, and topographical relations to the sea, and to mountainous ridges, considerably resembling the valley of the Mississippi. Its density of population, exceeding that of many European nations, forms a remarkable difference, although one which may not always exist. The latitude of Saharunpore, the most northerly point visited by the epidemic, is nearly the same with that of the city of New-Orleans.

The tropic runs through the upper part of the Delta of the Ganges, including Calcutta, within the torrid zone, and nearly striking the cities of Dacca and Nuddea. This insular district is in such a swampy state, as to be in many places uninhabitable, and the resort of tygers and other wild beasts, although it contains such large and populous cities. Calcutta is supposed to have upwards of a million inhabitants at present, doubling its population within the last twenty years.

To this district, the work before us is confined. The disease, however, extended throughout the whole peninsula, and has since penetrated to Arracan, Malacca, and the heretofore healthy island of Penang.

The alarm produced by the ravages of this epidemic, appears to have been awful. No accurate account of the number of deaths is afforded—but we may state that conjecture has estimated it within the space of three years, in millions.* Cities were evacuated, intercourse and trade suspended, and, as we learn from another source, the disease became the general plea of excuse for the non-fulfilment of contracts of every description.

This disease, the “mort de chien” of Curtis and Johnson, appears to be described by Aretæus, Cælius Aurelianus, Paulus Ægineta, and many later writers, including Bontius, Sydenham, Sauvages, &c. It has been generally known in India, as occurring sporadically, and as the most aggravated and malignant form of cholera. It had, previously to the year 1817, attacked small districts of country, and detachments of an army, with inconceivable fury and fatality. It,

* More than three millions and a half!

however, disappeared in a short time, either spontaneously, or upon the removal of the troops to a more healthy district, and never spread to the contiguous country. The local circumstances to which these people were exposed, have been, in general, amply sufficient for the generation of the disease.

The year, in Bengal, and northern Hindostan, is divided in common acceptation, into the cold, the hot, and the rainy season. The cold, from November to February—the hot from the second week of February, to the second week in June, and the rainy period, from this time till November. Towards the middle of October, the mornings and evenings begin to grow cool, and the wind, from being generally at the south and east, comes round to the north and west, and carries away the clouds and storms of the south-east monsoon. The weather, during the cold months, is described as delightfully clear and pleasant, with a cool, sharp, dry wind from the north, and sometimes from the north-west. December and January are foggy during most of the nights. The *cold* here spoken of, increases until January, and is termed piercing at 47° ;—the mean heat of the coldest month, is 68° . There are rarely a few heavy showers about Christmas, generally none during the season. This weather is remarkably refreshing to European constitutions, and even vegetation is said to be, at this time, in its greatest vigour.

In the hot season, the power of the sun is severely felt. A strong and steady wind blows from the south, driving dispersed masses of clouds, and the remaining fogs, in the opposite direction. They, however, return in violent and sudden thunder-storms, much like those so common in this country, and termed *northwesters*. The greatest heat is in May, and the thermometer ranges from 81° to 93° —mean heat, 86° . Sometimes “lesser rains” fall between the 15th and 25th of this month. Towards the 7th and 10th of June, the wind becomes east, cloudy, and with showers, marking the approach of the rainy season. Tremendous thunder-storms, with vivid lightning, a gusty, changeable wind, and alternations of heavy rains for a few days at a time,

with beautiful clear weather, and a manifest diminution of heat, mark this period. The rivers, wells, and tanks are speedily filled, and the lower part of Bengal is entirely covered with water, in many places navigable by boats.

Catarrhs, intermittents, rheumatisms, and diarrhœas, are common in the cold season: bilious remittents, during the latter part of the heats, and beginning of the rains, and dysenteries for the remainder of the rains. Acute hepatitis prevails at all seasons, but particularly in the early part of the rains. In the northern provinces, the usual result of difference of latitude is perceived in the greater coldness of the winter.

From 1815, to the middle of 1818, the weather deviated, in a remarkable degree, from its usual routine in the different seasons.

We shall not pursue it in all the detail in which our author has thought proper to describe it. Unusually heavy rains, occurring during the dry seasons—close, damp, uncomfortable weather, in the cold seasons—temporary and long continued dry spells in the rainy season, and other departures from the common and regular order of things, characterised the whole period to a remarkable degree, and produced an unusual number of bilious fevers, and an increase of cholera, during a part of the time.

This disease, an endemic of the country at particular seasons of the year, was more frequent than ordinary in the earlier half of 1817, but did not attract much attention till August, when it suddenly burst out at Jessore, near Calcutta, with such extreme violence, that the inhabitants immediately commenced flying in every direction, notwithstanding which, in the town and district, it is said to have cut off more than six thousand persons in a few weeks. It was afterwards found, that it had originated in Nuddea, in May, and in Behar, Patna and Sunergong, in July, in an epidemic form. In the course of August, it spread to Silhet, Chittagong, Rajshahy, Bhaugulpore and Monghyr. These places are scattered over an extent of about 450 by 200 miles, and in the four first named, no connexion be-

tween those very distant points was discovered. In Calcutta and its suburbs, it commenced early in August, increased during the month to an alarming height among the natives, and attacked Europeans early in September. In three or four weeks, "few towns or villages of any considerable size, wholly escaped its attacks," from Silhet to Cuttack, and from the mouth of the Ganges nearly to its junction with the Jumna; a distance of four or five hundred miles in each direction. A vast number of places suffered severely, and in the whole Delta of the Ganges, the population was sensibly diminished.

The epidemic now "began to affect particular lines, and to fix itself in particular divisions of country—wholly restricting itself for the time to the course of those lines and divisions." Leaving the province of Bengal, it confined itself for many months to the west side of the Ganges and Jumna—but after breaking out, in March, at Allahabad, situated at the junction of these two rivers, a new stream of pestilential virus, issued in various directions, and in regular succession, invading a great part of the tract previously untouched: and on the 6th, 7th, or 8th of November, it reached the centre division of the army under the Marquis of Hastings.

The rainy season, continuing till the end of October, suffered various irregularities, and was very different in different parts of India. We shall hereafter note the apparent effect of weather.

The mortality in the centre division appears to have been frightful. "The sick were so numerous, that the medical men, although night and day at their posts, were no longer able to administer to their necessities.—The noise and bustle almost inseparable from the intercourse of large bodies of people, had nearly subsided. Nothing was to be seen but individuals anxiously hurrying from one division of the camp to another, to inquire after the fate of their dead or dying companions; and melancholy groups of natives, bearing the biers of their departed relatives to the river. At length, however, there were neither time nor hands to carry off the bodies, which were then thrown into the

“neighbouring ravines, or hastily committed to the earth
 “in the spots on which they had expired. All business
 “had given way to solicitude for the suffering. Not a smile
 “could be discerned nor a sound heard, except the groans
 “of the dying, and the wailings over the dead.”

This took place in a body of about ten thousand fighting men, and near eight times as many persons constituting the camp followers of this Indian army. It is conjectured to have cut off eight or nine thousand. In about twelve days, the division having removed to an elevated position, strewing their path with the dead and dying, who dropt on the march, and those who left the carts, pressed by the sudden calls of the disease, and were unable to return, the mortality at length subsided.

A minute account is now given of its progress in different directions. This, however, as well as many other parts of the work, we must glance over. For several years previous to its commencement, as we have stated above, extraordinary deviations from the usual course of the seasons took place, of a character too various to be classed under any single head—and at the time of its first appearance, the air of the province of Bengal was in an excessively humid state. But the prevalence of the disease was under diversified circumstances in these respects. In the provinces near the sea, it lingered without ever entirely disappearing, and renewed itself from time to time—while, over the upper provinces, its route resembled that of an invading foe, carrying terror and destruction in particular directions, and sparing others, seldom remaining longer than from three days to two or three weeks in one place. It followed, in a very great number of instances, the course of the prevailing east or south-east winds, as if the virus were blown from place to place. It showed a marked partiality for the vicinity of rivers, and frequently passed by places situated at a very small distance out of its direct road. Towns and bodies of troops were sometimes, however, free from its attacks, though lying in the direct route, and not circumstanced in other respects so as to afford reasonable cause for security. It would after-

wards return, as if from a fresh origin, to the eastward, and range over large districts, or seize here and there on particular towns and posts that had previously escaped. It was very uncommon, though in some instances it did occur, for an individual, whose strength had been restored after an attack of the disease, to undergo it a second time. This was also conspicuously the case with towns and large bodies of men—those in which the disease had occurred to any extent, being rarely subjected to it a second time. Regiments, and other parts of armies, often escaped in an extraordinary manner, while intermixed with those suffering extensively. They were, notwithstanding, sometimes affected, and several too a second time, and particularly in the latter part of 1819. To have merely been exposed to the disorder, though not invaded by it, is said to have afforded considerable temporary security.

In situations at ordinary times healthy, those which were well ventilated, dry, remote from marshy exhalations, on elevated ground, in higher northern latitude, or with a population not in a crowded state, generally had the disease for a shorter time, and with less frequency and violence. Nevertheless, in particular seasons it would trespass over all these restrictions, and even sometimes single out elevated and airy points, sparing those in the plain below. Though generally restrained from crossing ridges of mountains, this was not always the case, and it entered, with violence, the province of Napaul, and penetrated the lower ridge of the great Himalaya mountains. It always, however, as far as is known, affected a crowded population.

It appears that the environs of Calcutta, as well as several other of the places where it was most permanently destructive, are surrounded with thick woods, in close vicinity with the buildings. These are even termed, at the metropolis, a "boundary hedge." The filthy condition of many of the above situations, would seem to be extreme.

As regards the season of the year in which it most prevailed, it is described, in almost every case, as partially or entirely disappearing during the winter—while at all

other times it seems to have been produced indiscriminately.

The great predisposing cause was the unknown one, that rendered the disease epidemic, and which all the researches of the Medical Board have been unable to discover. Among those of minor agency, debility from any cause, including other diseases, fatigue, and exposure to heat and cold, are enumerated. It is here remarked, what may in fact be with justice said of the greater part of any epidemic, that the predisposing and exciting causes alternately change places.

The principal exciting causes, with this qualification, as acting upon large bodies of people, are "enumerated in the following order, according to their degrees of priority and frequency of operation. 1stly, Alternations of heat and cold, combined with rain, or a very humid state of the atmosphere. 2dly, Simple alternations of heat and cold, without moisture." In individuals the disease was often immediately induced by various causes.

"Of this sort were the drinking of large draughts of good or bad water, when the body was heated; gorging after long fasting; eating food of a deleterious quality; going suddenly from a hot into a cold atmosphere; lying exposed to the wind, when the cuticular pores were open, and the perspiration was flowing freely."

A chapter is devoted to the question whether the disease is contagious. We are first told, as respects the communication of it by individuals, that the medical officers of Bengal are unanimous in the belief, which is also entertained generally by the people, that it is *not contagious*. A question, however, seems to have arisen with many, from the manner of its appearance in one or two particular places, whether it was not communicable by a detachment of troops. Our author enters minutely into the circumstances of the case, but it is not necessary to follow him. The results, which we think he has pretty fairly drawn from them, are, that the coincidence of the attack of cholera, with the arrival of the troops supposed to convey it, was accidental,

and that it was occasioned by general causes. Numerous examples are given in which it was *not* communicated.

An opinion is entertained in the body of the work, that this disease deviated from a law usually attributed to epidemics, in not expelling other diseases, nor converting them into its own nature or form. The ordinary complaints of the season are described to have been generally of their usual frequency and violence, and sometimes more, without any disposition to turn into cholera. In the Supplement, however, which we could almost imagine to have been written by a different hand, it is mentioned as "giving place to fevers and other complaints," and "giving way to violent remittent fever."

Brutes are said to have died in great numbers at several places. An elephant had every symptom of cholera, and was cured by brandy and laudanum!

We regret that our limits will prevent us, after going thus far, from presenting a sufficiently copious account of the remaining matter. The practical part of the work, including the description of symptoms, and of appearances on dissection, is full, and very ably written. We shall furnish a few abridged extracts.

"The attack was generally ushered in, by a feeling of fullness and pain in the stomach, and swelling of the abdomen, with sickness, and a desire to go to stool. Then came, almost immediately, vomiting and purging of a pale, thin fluid, without taste or smell; great anxiety; oppression, and a sense of constriction about the heart and præcordia; thirst and internal heat. These symptoms were accompanied or quickly followed by severe cramps; generally beginning in the fingers and toes, and thence extending to the wrists and fore-arms, calves of the legs, thighs, abdomen, and lower part of the thorax. The action of the heart and arteries was uniformly diminished. The pulse sunk rapidly at the wrists and temples; and at last could no longer be felt, or was merely perceptible by a slight and indistinct fluttering. The respiration became laborious and hurried, with sighing, and frequently broken inspira-

tions. The skin grew pale, shrunken, and cold; clammy, dank, and disagreeable to the feel; bedewed with large drops of cold sweat, and discoloured of a leaden, bluish, purple or livid hue. The features were contracted, collapsed, and ghastly. The eyes sunk in their sockets, fixed, and glassy, covered with a thick film, suffused, and surrounded by dark-brown or black circles. The mouth was dry and parched; the tongue bluish or white, and faltering; and the voice hoarse and low." It is hardly necessary to add, that the prostration of strength "was sudden and great." "In feeble habits, and in attacks of extreme violence, the scene was soon closed. The patient fell into a listless state, and had no longer strength for either full vomiting or purging. A little fluid was now only rejected by the mouth, as the abdominal muscles were thrown into spasm; or passed off involuntarily downwards, as the *body* turned round in bed. The patient insensibly sunk in death, or was carried off, during a repetition of spasms, within four, six, or twelve, and sometimes, in one hour. This state of collapse sometimes lasted much longer," but "almost certainly portended death."

Much variety occurred in the kind, order and sequence of the symptoms. Vomiting was generally the initial symptom, though frequently purging or spasms. In some cases "the patients fell down, as if struck by lightning, and instantly expired." The fluids discharged by vomiting and stool, were enormous in quantity, and generally watery and tasteless, but sometimes sour, green, dense, like infusion of tea, starchy or mixed with mucus, and viscid. In no instance was feculent or bilious matter passed off in the commencement of the attack.

After from twenty-four to forty-eight hours, and sometimes three days, if life escaped, the stage of collapse terminated—with natives of the country, in perspiration and sleep—with Europeans, in the symptoms of bilious fever. In some instances partial returns of warmth, with other favourable signs, were followed by sudden relapse and death. Chronic irritability of the stomach, with disor-

dered bowels, were common consequences, and sometimes incurable dropsy, and partial loss of hearing or sight,—and, in one case, palsy of the bladder and lower extremities. The earlier that dark feculent or vitiated bilious stools were procured, the more favourable was the case. The *chocolate coloured fluid, with flocculi swimming in it*, now and then occurred, and as we may readily suppose, was generally a fatal omen. Large quantities of black bilious matter were sometimes passed off by purgatives, taken during a state of derangement of the alimentary canal, often a prelude to the disease. In the vast extent of country over which this disorder was distributed, it is natural to suppose it was various in its forms, which, accordingly, are described in detail, and of which, we regret, that we cannot give any further notice. In some places, it approached the common cholera of the United States.

The appearances on dissection were diversified. The bodies of those who died in a very short time, presented scarcely any vestige of disease. The stomach and intestines were pale and relaxed, filled with whitish or muddy fluid, or with air. Where the disease had existed for some time, and especially in vigorous subjects, strong marks of an increased quantity of blood were found in the viscera. The intestines varied from a rose colour to a dark hue, and the stomach sometimes exhibited, externally, distention of its vessels. This viscus was much contracted, hard, and frequently thickened in its coats, highly inflamed on its inner surface, sometimes coated with coagulated lymph, and sometimes ulceration and mortification were commencing. The intestines were irregularly distended and contracted, with frequent intussusceptions and adhesions. Their contents were fluids similar to those ejected, and in prolonged cases, “a dark, thick pitchy stuff.” The liver was mostly enlarged, and gorged with blood, flowing profusely whenever the organ was wounded—sometimes large and soft, or collapsed and flaccid—the gall-bladder generally full of dark green or black bile—sometimes empty, or flaccid with light coloured contents. In some cases, the liver and gall-bladder had no

signs of disease—the spleen, vena-cava and vena portarum, with their branches, heart and lungs, “were stuffed” with blood—the lacteals turgid with chyle, so as frequently to have a tortuous or knotty appearance—the œsophagus sometimes inflamed and ulcerated in its inner surface—the bladder now and then inflamed, generally much shrunk—the brain mostly natural, though sometimes with marks of venous congestion, or incipient inflammation.

The practice in this disease, was, if possible, still more various than the phenomena. The following results, however, are drawn from the several reports handed in. The attacks were, in some instances, of such violence, as to put it out of the reach of human means to save the patient. But an evident difference, in some cases, to a considerable amount, in the number of deaths, was produced by the proper employment of remedies. A material advantage was gained by an early call of medical attendance. “In Europeans generally, and in robust natives,” bleeding could commonly be practised, to the amount of from twelve to twenty or thirty ounces; and where this could be done, it was decidedly the most useful remedy employed, “cutting short the disease, resolving spasm, allaying the gastric and intestinal irritability, and removing the depression of the system.” It should be recollected that in those cases in which this remedy was of so much service, the pulse was already small and rapidly sinking, and the heat and muscular strength of the system in the same predicament. A very common practice indeed, was to tie up the arm, and bleed, if the blood could be made to flow. But in the generality of natives, complete collapse took place so soon, as to render venesection impracticable from the beginning.

In such cases, and in addition to bleeding in those above-mentioned, the cure was best attempted by “dilutents, powerful anodynes and stimulants, combined with calomel, and followed up by mild laxatives and tonics.” It is not affirmed that calomel displayed any specific power in checking the disorder, but it soothed irritability, and “was per-

“ haps of more certain sedative operation than any other “ medicine.” Cantharides did not vesicate in time to be of material use. They were often imitated in their effects, with advantage, by applications of boiling water. Balls of cotton, dipped in spirits of turpentine, were sometimes burned over the region of the stomach. The warm bath was unmanageable.

Such is this epidemic, which will be as memorable in history, as the strange and destructive plagues of antiquity. Though subsiding in Hindostan, it continues to spread, and has reached the distant islands of Pulo-Penang and the Mauritius. May the fear of its extending to America, prove unfounded!

The style of the work is plain, and sufficiently good, with the exception of a few vulgarisms. Its extremely condensed form, being in itself an abridgment, greatly increases the difficulty of making an analysis. We have, however, endeavoured to give as fair a one as was in our power, though of course at the expense of any advantage arising from greater copiousness.

ART. XX. *The Pharmacopœia of the United States.* By the Authority of the Medical Societies and Colleges. Boston, printed by Wells & Lilly, for Charles Ewer, No. 51, Cornhill, 8vo. pp. 272. December, 1820.

THE American physicians at length are favoured with the long expected National Pharmacopœia—the result of the wisdom of the united representatives of every part of this vast republic. Appointed by the district conventions, the general convention of medical delegates, after a full and perfect intercommunication of opinions, has given this fruit of its labours to the public, and it, of course, is now open to investigation, and is to be judged of by its intrinsic merits.

In framing this important work, although chiefly a transcript of the British Pharmacopœias—having adopted that

part of them which was deemed most appropriate, the convention has identified it with itself—and is consequently responsible for it in every particular. As the Medical Republic of United America is to be governed by its authority, it becomes of the greatest importance to expose it to the most rigid scrutiny, in order to ascertain how far it is deserving of confidence—and to detect its errors if any exist. That it will be critically scanned by the reviewers abroad, we cannot for a moment doubt. Looking upon every thing American with no partial eye, they will gladly avail themselves of this opportunity of estimating the pretensions of a work, which we are informed “after being gradually matured by the advice, consent and co-operation of bodies of physicians in all parts of the Union, is at length committed to the press, as the result of their deliberations and decisions.”—It is, on this account, particularly incumbent, that we should ourselves endeavour to ascertain what standing it ought to maintain amongst us—and that any defects may as soon as possible be amended. We are well aware of the difficulties attendant on the first production of a work like the present, and are consequently, willing to make every *reasonable* allowance for any imperfections we may discover. Constructed, however, as it has been, by the highest authority, circumstances, which coming from an individual might be regarded of little importance, become magnified in proportion to the rank it assumes, and the character of those from whom it emanates.

It is now many years, since the physicians of the Union were earnestly called upon to adopt some general plan for the preparation of their compounds, and for making a judicious selection of the simple articles employed in medicine. Desirable as such an event might be, no measures were adopted to carry it into effect, until in 1817, a project was submitted for the formation of a National Pharmacopœia, by the authority of nearly all of the Medical Societies and Medical Schools of the country.* This plan is amply detailed in the historical introduction to the present work; from which it

* Neither the University of Pennsylvania, nor any medical body of this city, except the College of Physicians, was represented on this occasion.

appears that the general convention of medical delegates, assembled in the capitol at Washington, on the 1st of January, 1820, and having elected the requisite officers, proceeded to the adoption of an *American Pharmacopœia*, which was ordered to be published by a special committee, appointed for the purpose.

A work of this nature, however auspiciously commenced, cannot be regarded as ever arriving at perfection—since the march of scientific research will necessarily require its frequent revision. Aware of this, sundry resolutions were adopted by the convention, previous to its adjournment, in order to secure a meeting of delegates for 1830—and a judicious clause was inserted, namely: that “Whereas the progressive improvements in medicine, *as well as other causes*, may render it expedient to revise the *Pharmacopœia* at an *earlier period* than is expressed above; it shall be the duty of the president, to call the attention of the medical societies and colleges to the subject, whenever, in his opinion, the public good may require it.”

Perfection is not the work of a day. Had this *Pharmacopœia* been the first of the kind ever promulgated, it might have been regarded as of the highest order: but standing as it does, on the shoulders of those which have preceded in the same career, it was reasonably to be expected, that if nothing of real importance was added by the convention, the errors and mistakes of predecessors, should at least have been avoided. Though the wisest of men has affirmed, that in a multitude of counsellors there is wisdom—without any derogation to individual merit, possibly many of the readers of the work will exclaim in opposition to this, in the trite language of a well known proverb.

The probability is, that *an earlier day of revision may be deemed necessary*, than that pointed out. Ten years is a long period to look forward, if error exists which ought to be rectified. It is presumable, and certainly proper, that after perfecting it as much as possible, a decennial revision at least, will be fixed on to modify a work, which

must always be intimately connected with the progression of that science it is intended to assist.

In a preliminary discourse, the objects of a pharmacopœia are first pointed out—the justice of which we shall not contest. We must, however, advert to the extreme difficulty it embraces—since the utility of few medicines can be considered as being either fully established, or well understood, although affirmed to rest on the basis of experience—judging, at least, from the contradictory evidence in respect to almost every article of the *materia medica* ! A medicine strongly recommended by some, is equally opposed by others—or, if now acquiesced in, a very few years promotes a revolution of opinion, which tends to weaken greatly our previous estimation of its merits.

It is affirmed, moreover, that the value of a pharmacopœia depends upon the *fidelity* with which it conforms to the best state of medical knowledge of the day. From this position it is believed few will dissent—though probably, the succeeding sentence will not be so readily acquiesced in, viz. that “its *usefulness* depends upon the sanction it receives from the medical community and the public ; and the extent to which it governs the language and practice of those for whose use it is intended.”

The present work may be considered as having received by *anticipation*, the sanction of the profession at large—since they have agreed to be bound by the actions of the representatives selected to bring it forward—and to maintain it, until any requisite change may be regularly effected. It, however, by no means follows, that its usefulness is commensurate thereto—and yet, the extent of its government over the language and practice of the profession, is bounded only by the limits of the United States.

It is further stated, that “in most *European* countries, works of this kind have appeared under the authority of medical colleges and corporations ;” and that “their usefulness has generally been co-extensive with the influence of the bodies of men, from whom they have originated.” To explain why they have been less useful than might have been hoped

for, it is asserted, that "it is because different works of this kind, proceeding from different sources, and disagreeing with each other in their details, have been permitted to circulate in the same community," &c. This, if correct, would sufficiently establish the position: but, it is believed, that in no European country, where a regular pharmacopœia is sanctioned by the authority of government, is any other than that one permitted to appear, that is to say, for general use. Hospitals and private institutions have, indeed, their local pharmacopœias, which are confined to their individual precincts. Great Britain, it is true, has no less than *three* pharmacopœias—each of them, however, appertains exclusively to a separate part of that dominion, proceeding from colleges totally unconnected, and which own no obedience to one another. Thus the London Pharmacopœia is confined to England, properly so called—the Edinburgh, to Scotland—and the Dublin, to Ireland. It is the same on the continent itself—and medical compounds are prepared in every place, by such authorized standards. Their usefulness *has not at all* depended on the alleged influence of the bodies from which they originated—but from a conviction every where felt, that a *fixed* standard of preparations, must be of more benefit to the practitioner, even if imperfect, than the uncertainty which exists in practice from diversified formulæ. An absurd pride, has hitherto prevented the three British colleges from producing one pharmacopœia only for common use amongst them—and it has been lamented by more than one person, whose views were not narrowed by local prejudices. But whoever is acquainted with the feuds that have existed for years, between the members of the London college, and those physicians who have been refused admittance into it, from having graduated elsewhere than at Oxford or Cambridge, will not lay much stress upon any real influence which that college may be presumed to possess over the majority of English physicians.

It is this uniformity of preparations, which alone renders a pharmacopœia desirable: it is this which principally sug-

gested the propriety of the one now presented to the public, and which will give it a standing amongst us, even if not equal to our anticipations.

The variable nature of the medicinal preparations throughout the Union, in consequence of every one being left to his own discretion in selecting from the numerous publications, both foreign and domestic, is alluded to in the preface, and the resulting evils are well exposed. It was the conviction of this fact, that long ago led to numerous attempts, by the College of Physicians of this city, and the Academy of Medicine, &c. to establish one general system of prescriptions—ut which, from causes difficult to unravel, unfortunately were never completed. It was the difficulty arising from a diversified pharmacy, that led at length to the formation of a dispensatory here, which selecting only one of the many formulæ existing, without determining on their respective merits, it was hoped, would meet the approbation of physicians and apothecaries, although emanating only from an unauthorized individual. In order to effect it to the best advantage, it was deemed proper to ascertain, as far as possible, which of the British pharmacopœias was most generally employed. It was, however, soon discovered, that no unanimity prevailed in this respect. Some persons preferred the one, some the others—whilst private, and altogether unsanctioned prescriptions were not uncommon, even for preparations of high importance. This being the case, a selection was made in conformity to the best judgment of the editor, and was embodied under the name of the American Dispensatory.

Other works of a similar nature, were afterwards given to the public, which, although highly valuable, nevertheless tended again to restore the evil, which was beginning to disappear. *Dissimilarity in the formulæ*, effected the same confusion which had originated in the indiscriminate employment of the three British pharmacopœias. It cannot then be doubted, that the proposition for forming, by general consent, one regular and uniform standard, would meet the instant approbation of every physician who had the suc-

ness of the profession at heart. No measure, accordingly, was ever more promptly acceded to, as is seen by reference to the historical introduction of the work before us.

A deliberate examination of that work has, however, satisfied us, that any one of the gentlemen engaged in the task would *singly* have produced a more perfect compilation than has been accomplished by the conjoined labours of the whole. Contending views and conflicting opinions, were to be reconciled—which could not be done without detriment to the work itself—and hence it is to be feared, that it will be regarded as inadequate to the momentous objects it proposed to attain. We may be wrong in our opinion, but conversation with others has satisfied us, that we are not singular in it—and we are yet to see from other quarters, what is generally thought of it *as a national enterprise*.

In expressing our sentiments thus freely, we do not mean to convey a doubt of the learned members having, as they assert, “on each of its departments endeavoured to bestow that degree of careful inquiry and mature deliberation, which the importance of the occasion demanded.” It would be absurd indeed to imagine any intentional omission on their parts, when both individual and national character was implicated in the result. It is trusted, therefore, they can say, with Dr. Hope of Edinburgh, in his “Remarks on Mr. Phillips’ Analysis of the Pharmacopœia Collegii Regii Medicorum Edinburgensis,” “that *many trials were made of the different processes* directed in the most esteemed pharmacopœias in Europe, with the view of ascertaining their comparative merits, and that *those adopted by us have in general been many times repeated*.”*

In all countries where medicine is regularly pursued, it seems generally admitted, that the Materia Medica in its lists, has erred rather from a redundancy than from a paucity of materials. To this, the preface naturally adverts,

* Annals of Philos. vol. i. New Series.

and adds, what the experience of every physician confirms, that "the number of articles necessary for the management of diseases, and especially of those which any individual physician *actually* employs, is always *very far* short of the catalogue afforded by most pharmacopœias." We entreat every practitioner here, to pause, and to note down the articles of the *Materia Medica* which he makes use of in his own practice! If such a measure be generally pursued, and lists are sent to the next convention, or to any person it might appoint for the purpose, it would readily be discovered which of the articles met the *universal approbation* of the profession. Let it be agreed on that none shall be adopted as a standard remedy, which did not receive the sanction of at least a majority of the returns thus forwarded. Beyond a doubt, our present lists would greatly diminish—and, we think, with a commensurate utility. This, or some similar measure, is in fact the only way of ascertaining the general opinion on this point—and if, laying aside all local prejudices, the medical men of the United States would agree to thus simplify the national pharmacy, by settling definitively the raw materials to be retained, we cannot but imagine the most favourable results therefrom. By doing this, they are not restrained in their individual researches, as to the merits of new or local remedies. If individual experience is favourable to any new article, let that experience be detailed and transmitted to the general representation when assembled. By them approved, let their recommendation lead to further trials by the profession at large—and should those trials substantiate its claim by the next decennial meeting, let it be received into the revised Pharmacopœia as a standard remedy.* All precipitation is hereby avoided, and the sanguine views of individuals will be thus corrected. Trifling and inactive articles will in this way be set aside, and confidence placed in those retained. A measure so obviously replete with utility, will confer an honour on the

* It is with medicines as with books, and the adage of *nonum prematur in annum* is equally applicable to the introduction of new remedies.

country that shall first adopt it. Is it not notoriously the case, that where numbers of superfluous articles are separated from the lists of the *materia medica*, whenever a pharmacopæia is brought forward, an equal number of useless articles is added to it?

We wish it were in our power to admit the justice of the remark made, that the omissions in the present work, were only of articles considered inert, or where they were abundantly superseded by substitutes more powerful and more accessible. "The system of retrenchment," it is added, "*might, no doubt, have been more rigorously exercised, without ultimate disadvantage to the interests of medicine.*" But it was thought to be at present more conducive to the public good, to retain on the list all those medicines which were believed to be so much in use in any part of the United States, that their omission would occasion inconvenience to physicians and apothecaries, and render the book less applicable to their wants."

To us, at least, the reasons thus assigned are by no means satisfactory. We may apply to it the language of Bergman to Morveau, on the subject of the change of chemical nomenclature, when he bids him to "spare no improper names. Those who are learned, will always be learned—and those who are ignorant, will thus learn sooner."

In reference to the point in question, it must be recollected, that the persons selected to prepare this great national work, had no restrictions of any kind imposed upon them. It was hence their duty to have an eye to *general* utility alone, without the slightest attention to the trifling, and temporary inconvenience of individuals.—By the avowal above made, they evidently admit, that they have not effected the purpose of their appointment to the best of their judgment.

In an excellent communication in the 15th volume of the *London Medical Repository*, p. 177, by Mr. Heineken, the following observations occur, which strike us as very applicable to the present subject.

"These appear to be some of the reasons for the great

contrarieties which occur in the opinions and practice of medical men ; and account, in some measure, for remedies being for a time considered never-failing, and then, ineffectual.—*Fashion*, also, has considerable weight in our profession ; and I believe the majority of my medical brethren will join me in decrying its interference, and in the opinion that fashionable remedies, to say the least of them, are seldom worth much. Their fame is generally merely ephemeral, and after working wonders for a time, they sink to their just level, and are estimated according to their intrinsic worth. I remember at one time hearing of the great virtues of the *pyrola umbellata* ; but it was soon found that squills answered just as well—*digitalis*, a great deal better ! *Piper cubebo* was, not long ago, so much the rage, that a druggist actually let me have a pound (and at a very exorbitant price too) as a favour ; but in the course of my limited practice, I have found it at best uncertain—in some instances nearly inert, and in some, painfully active.

“ The liquor arsenicalis was, not long since, announced as a perfect panacea. It has now become a highly useful, but by no means infallible medicine.

“ Prussic acid has been extolled, not only for its wonderful effects in phthisis, but also in stomach complaints (rather opposite maladies) ; but some are already beginning to class it with *digitalis*, and the whirling table, in the former disease ; and to attribute the cure of the latter, as much to the plan of diet pursued, as the medicine given.

“ Several of my friends are, at this moment, almost working miracles with the *vinum colchici*. It cures rheumatism, gout, tic douloureux, nay, even gonorrhœa in their hands ; but with me, its failures have been at least equal to its successes.

“ Now, I do not mean to deny that all of these preparations may be very useful—my only objection is to the mania which constitutes them omnipotent, and renders them in many instances decidedly deleterious ; and I cannot help thinking, that he who knows a disease thoroughly when he sees it, and understands the principles upon which it should be treated,

will find the present pharmacopœia quite bulky enough for his purpose. Far be it however from me, to decry innovation—it has been the origin of the greatest good in physic, as well as in every thing else : but innovation for the mere sake of innovation, is absurd ; and using a particular remedy as we would wear a particular hat, because it is in vogue, still more so.”

As to what is said respecting indigenous vegetables, it may be in part admitted, as founded on correct views of the subject ; though when it is added as a reason for introducing some to the rank of *standard medicines*, because they were *much employed*, we must protest against the position. The question is not whether they be *much employed*, but whether in the deliberate opinion of the convention, their merits were fully and satisfactorily established. Much as it may be desirable to prefer our native productions, this is at best only of secondary consideration. Unless the virtues of any native article are from experience shown to excel, or at least to equal a corresponding one of foreign origin, such an article is not deserving of admission into a national pharmacopœia, as a standard remedy. As physicians, engaged in a continued warfare with death, we require the *best* article of its kind, if we are duly sensible of what we owe to our patients. The physician knows no peculiar locality for his drugs. Wherever best obtained, that only is to be kept in view, without reference to price, or any other motive, save the real interest of the sick. Either medicines are useful, or they are not—if they are, then they can only be so in proportion to their intrinsic powers ; and provided they be good, the practitioner is not peculiarly interested from whence they come.—In fact, it is conspicuous, by referring to the catalogue of standard remedies proposed in this work, that the proposition maintained is only partially conformed to. We have, for example, both Jalap—and its reputed equal, the Podophyllum ; so likewise, the Ipecacuan, and the Gillenia Trifoliata. Now, if both the articles of domestic growth, are equal to the foreign, these last are unnecessary ; and, being inferior, they did not deserve admission !—Many

other articles might equally be mentioned, but the above examples are perhaps sufficient.

In forming a secondary list, of *doubtful* efficacy, it is conceived that injury will rather accrue; although it is professedly given, for the purpose of prompting "to *farther investigations* into the character of the substances in question."* Whatever may be the real merits of the *new substances* introduced, but not as yet fully tested, we may with some confidence affirm, that of the rest, a large proportion has swelled the catalogue of preceding pharmacopœias, for nearly a century past—and if, in that lengthened period, their merits have not been made to appear, their present continuance is not likely to increase confidence in them. We would ask, whether much doubt exists, as to the real value, as medicinal agents, of such articles as Angelica, Calamus, Carota, Carthamus, Contrayerva, Curcuma, Iris, Monarda, Origanum Petroselinum, Rubia (Sambucus), Santalum Veronica, and many others mentioned! Are there not in the primary list, articles sufficiently powerful to at least supersede the necessity of the above? Why then crowd the already overflowing catalogue with inferior articles!—nay, even in the primary list of "*articles of decided reputation*," how many are there, which will obtain the *decided approbation* of every practitioner! We fear that many must sink into the tomb of oblivion, or at best be removed into the secondary ranks. What shall we say of the "*decided reputation*" of Anisum, Carum, Coriandrum, Crocus, Cubebs, Ficus, Fœniculum, Inula, Lavandula Pruna, Rosmarinus (Sambucus again!) Sarsaparilla, Spongia,† Tama-

* This may be highly proper in a Dispensatory, but in a national Pharmacopœia, it is submitted with confidence, that *no doubtful* remedy should gain admission; before it attains so exalted a standing, a complete development of its powers should be attained. It is the practice here recommended, which loads our shelves with articles far inferior in value even to bread pills! and which, when once introduced, are with difficulty driven from their situation, even when experience has tested their insufficiency.

† Perhaps this may, however, prove an useful remedy. It is one of long standing—but the cause of its probable utility has never until within a few months been suspected. Mr. Straub has ascertained, to a certainty, the existence of

rindus, Uvæ !—Many are certainly agreeable articles of food, and others may serve as condiments, or preserves, &c. But in vain shall we look for any high character in them for medicinal powers !—True it is, that most of them are imitates of foreign pharmacopœias ; though even there, they do not rank in great estimation ; and few practitioners will ever depend on such feeble resources.

It would seem, as if physicians considered every article to possess medicinal virtues ! We leave it to their better judgment to determine, whether the Almighty did not intend the far greater number, as *food only*, for man, and inferior animals. The late Dr. George Fordyce expressed his opinion, that whenever a substance was good for nothing else, it was tried in medicine. This failing appears to be equally the catholicism of the present day !

In an analysis of Dr. Paris' Pharmacologia, in the Annals of Philosophy, the editor, adverting to that writer's enumeration of the causes of the revolutions that characterize the history of medicinal substances, adds, " Now to this truly formidable list of mischief-producing causes, I think the author might have added, the *careless revision* of national pharmacopœias ; unless this may be classed under the head of *the ignorant preparation* of medicine " !

We owe, indeed, something to the convention for its rejection of many articles, which yet equal in efficacy a number of those retained ; the expurgation is, however, totally insufficient ; for we cannot believe that many practitioners will admit one half of those retained to be possessed of such superior merit as to entitle them to the distinguished character of *Standard Medicines*. Excepting for the purposes of communicating taste or smell to some of our prescriptions, for which three or four would have sufficed, we think they might have been for ever consigned to oblivion.

iodine in it. Iodine has lately been itself commended as an efficient medicine. —This work was, it is believed, adopted in its present form, before any notice of the above fact appeared. If iodine is the efficient part, it may readily be prepared from the sea weed of our coasts, and may well supersede the use of sponge, into which it enters in very minute proportion.

As to what is said respecting "that part of the work which contains the formulæ for the preparations and compositions," and of which we are told *those only have been preserved* "which have received the sanction of the faculty in this country or in Europe," we shall have occasion to show that this assertion is by no means correct. We do not say that some of the formulæ prescribed are inferior to others omitted, but which have obtained in Europe an equal sanction—inasmuch as this may be regarded as individual opinion—and will perhaps be differently considered by different practitioners. But we will only at present point to the preparation ordered for one of the most important articles in medicine, viz. *tartar emetic*; which is given on the authority of neither Europe nor America; being dependent alone, we believe, for its admission, on what has been stated by Dr. Phillips of London. We will ask the convention if any one of them have ever made the tartar emetic by the process thus recommended? We have, perhaps, a right to ask, which of the different formulæ proposed for our adoption has actually been submitted to the experimental researches of the convention or its committee! As it respects the formulas fixed on by different authorities for the emetic tartar, practical men will differ undoubtedly. Perhaps, with proper care, all will yield a perfect salt. Yet we still think, that if, after several years have elapsed since Mr. Phillips' plan was proposed, not one of the British colleges in the revision of their pharmacopœias has thought proper to adopt it, it might have been the subject of more consideration and actual experiment, before the convention decided on its admission as the best. We say nothing of the greater expense of this process, in consequence of the use of the *metallic* antimony, as this is in fact a secondary consideration.

Continuing their remarks, we are told, that the "Compound substances which are prepared in the large way at manufactories, and which are to be kept by the apothecary, though not necessarily prepared by him, are inserted on the *Materia Medica* list. Those which are to be *made by the*

apothecary alone, are placed amongst the preparations and compositions."

It certainly was proper that every article which, either simple or compounded, in *any way constitutes a part of any of our standard preparations or compositions*, should have a place on the list of medicines, as given in a pharmacopœia—and it is equally proper that every compound should have its *formula accurately laid down, whether it be made by the manufacturing chemist or by the apothecary* strictly so called. Without such *precise directions*, how are we certain that the medicinal preparations of Georgia and Maine, or even of two contiguous states, are similar to one another? But we will now ask if the convention really imagined that many articles, which appear "amongst the preparations and compositions" are generally prepared, or if, indeed, they ought to be prepared, by the apothecary, in the more limited acceptation of the name? We will venture to say, that whoever considers the subject as it deserves, will at once acquiesce in an opposite opinion. However qualified some of them may be as manufacturing chemists, &c. to effect this purpose, we are persuaded that the majority of them would acknowledge their total incompetency to accomplish the intention. We will ask the convention, then, to point out the apothecary in the United States, who, independently of his extending his pursuits as above mentioned, ever pretends to make the ammoniated alcohol; ammoniuret of copper; benzoic acid; black sulphuret of mercury; calomel; the carbonats of ammonia, potass, and soda; cinabar; crocus of antimony; corrosive sublimate; diuretic salt; ether; ethereal oil; flowers of zinc; glass of antimony; grey oxyd of mercury; hydrosulphuret of ammonia; lunar caustic; magnesia; muriats of ammonia and iron, of antimony, or barytes; the various essential oils; oxyd of antimony, and zinc; phosphat of soda; caustic potash; Rochelle salt; red precipitate; soluble tartar; sulphat of potass; tartar emetic; turpith mineral; aqua ammoniæ; white precipitate, &c.? Of these, how many apothecaries of their acquaintance would the convention be willing to

admit as competent to the task of preparing them? Is it not sufficiently known, that, for the most part, these preparations are the result of peculiar manipulations in the hands of *practical pharmacy*—with which very few of our apothecaries ever trouble themselves! Indeed, we shall find the formulæ themselves, as laid down in some instances, miserably defective—and evincing the necessity of having, in a convention for such important ends, men of *practical information* in the concerns which come before them. We shall advert at present to that important article *ether*, which ranks high in the estimation of every physician. What shall we call the ether of the present work? Selecting the formula of the London College for its preparation, the convention has only *pursued it in part*, and have neglected to give it to us in its state of purity, as effected by the London and other colleges, through the process of *rectification*! As it is, we have undoubtedly nothing more than an imperfect mixture of alcohol and ether, &c. and about on a footing with the *spiritus vitrioli dulcis*! Let us add, that *their* spirit of sulphuric ether (sweet spirit of vitriol) will be found to consist of nearly three-fourths *alcohol*! If such is the medicine we are to employ as ether, under the sanction of the convention, we forewarn all practitioners to be prepared for disappointments in its use.

What sort of preparation is that which, under the name of “Infusion of Peruvian bark with magnesia,” directs us to take

Of Peruvian bark, in powder, one ounce.

Magnesia, one drachm.

Cold water, one pint.

Add the water gradually, and rub them well together for fifteen minutes. Let the infusion stand for one hour, then *filter*.

What becomes here of the magnesia, and with what intention was it added, if to be removed by filtration? Of what utility is it, even if it were retained in the infusion? In fact, admitting the correctness of the prescription itself,

is it not rather one adapted to extemporaneous prescription, than to hold a place in a standard pharmacopœia.

We must be allowed to add, that, with the directions given to most of the formulæ, few persons, unversed in the practical details of pharmacy, would be able to form many of the preparations. They are defective in precision on many occasions, and will never be followed by the manufacturer!

In the preparation of the tincture of bark, we are not told which of the species we are to employ, of the three that are introduced into the list of medicines.

The prussic acid, as prepared by the convention, is merely that of the illustrious Scheele; and very incorrect in a most important part of the process. What shall we say of an ointment of *rose-water*! in which the water must necessarily escape, leaving only a simple cerate slightly impregnated with the odour of the rose. It is, at best, a mere *lip-salve*, and unworthy of a place in a national pharmacopœia. Who, that knows any thing of Warner's celebrated gout-cordial, ever heard of its preparation without rhubarb!

Sed, cheu! jam satis est!

We come now to advert to what is said of *nomenclature*. No one will doubt of the difficulty of this part of the subject, since the diversity of names given by different pharmacopœias to the same article, abundantly proves the fact. But that the one adopted, conforms, as we are told, to the present language of science, we cannot admit, at least, in every particular. To notice only the terms of *submuriate* and *oxymuriate* of mercury? How are these terms reconcileable to the views taken in chemistry of the substances they are intended to designate? Whether they be regarded as compounds of muriatic acid, and the oxyds of mercury, or of chlorine and mercury, the terms are obviously alike incorrect. The one is not a submuriate, neither is the other an oxymuriate. It is true, the names are sanctioned by the London Pharmacopœia—but that they are not legitimate offsprings of scientific research, is admitted by Dr. Powel himself, the translator of

that work.* We must rather consider the terms adopted by the London College, as the result of habit, and a wish not to alter the names of substances, difficult to discriminate, which, although erroneous, had been for a considerable time employed. Such views ought not, however, to have had any influence with the framers of a *new pharmacopœia*, expressly formed for our own country. There is no excuse for retaining old names, if improper; when so good an opportunity offered of removing the trammels of other countries, which cannot be supported on philosophical or scientific principles. The above remarks are more deserving of consideration, inasmuch as, without any apparent reason, other names are changed by the convention, of less importance, but by which the articles were better known, and this without any adequate benefit. Such are the *tinctura camphoræ opiata*, instead of *tinctura opii camphorata*! *Mel scillæ compositum*, instead of *syrupus scillæ compositus*, &c. If, as the convention justly say, "the essential properties of names ought to be expressiveness, brevity, and dissimilarity," wherein, we would ask, are the terms of submuriate and oxymuriate, to be compared with the old names of calomel and corrosive sublimate? These last have the advantage of being *altogether unconnected with every theory of the past, the present, or ensuing times*! One of them is, moreover, expressive of the poisonous nature of the article. In *brevity*, they surpass the names adopted by the convention, and certainly, there is less similarity, from which danger is always to be apprehended. If it be said, in opposition to these remarks, that calomel does not imply a *white* substance, such as that it is intended to substantiate—neither do the terms of oxygen, hydrogen, and hundreds more, signifying what they were at first intended to designate—but which, having been consecrated by time, it would now be injudicious to alter, because no evil arises from their being continued in use.

The changes announced in the nomenclature of articles derived from the vegetable kingdom, are probably correctly

* Edition 1809, p. 129.

instituted—at least no injury can arise therefrom. Time will, however, demonstrate, whether the best plan is herein adopted. Although we say no injury can arise therefrom, we may be permitted to express an opinion, (not now for the first time suggested,) that the continual fluctuations in scientific names, is one of the greatest sources of the difficulties attendant on the acquirement of knowledge. The immortal Shakspeare says, “a rose by any other name will smell as sweet,” and we may say, that jalap, &c. by any other name will purge as well. All this is true. Yet if names are perpetually changing, either from fancy, or some presumed superiority, how is the learner of our science to ascertain precisely the substance intended—without a perfect knowledge of all the synonymes to which it has given origin? But does the knowledge of *words*, improve his information as to the properties of the thing they all equally express? Is not his time uselessly expended, which might otherwise be advantageously employed? Thus in the present work, the appellation of *acacia gum*, for gum Arabic, is no improvement, although sanctioned by the London College. For centuries past, it has been designated by the latter term, and we are now compelled to learn new terms for common articles, which, in a few years, may again receive a change. These innovations, like fashions in dress, may be considered as the trappings of science, and are beneficial only to the bookseller and the printer. But while animadverting on the term of *acacia gum* in place of gum Arabic, which it is intended to designate, we must not, from any undue tenderness, neglect to state, that by old authorities, the term *acacia*, was meant to express something altogether different from gum Arabic. *Celsus*, who is, we presume, good authority, speaks of *acacia*, and also of *gum*, by which, his annotator assures us, he always meant *gum Arabic*, when thus mentioned singly. *Salmon*, and other authors of his day, speak of *acacia*, as being the *juice of the unripe seed* of the plant—whereas, gum Arabic is an exudation from the bark, and altogether a different thing.

This, however, is a trifling error, and if it stood alone, we should scarcely have deemed it worthy of any remark. But what reply can be made to the inquiry, how the convention came to adopt the name of *aqua potassæ*, to *two* preparations of *infinite difference*!!! Let the reader look at page 82, of the Pharmacopœia, and he will find a *pleasant beverage* under this title. Let him next turn to page 188, and he there finds, under the *same name*, an article, which, if given for the other, would be productive of death to the person who should swallow it. The first a *weak solution* of *super-carbonated* potash—the last, a *strong solution* of *caustic* potash: but, no doubt, the *modern language* of chemistry is here strictly followed!

Perhaps the Works of Homer, *Travestied*, may have chanced to meet the eye of some member of the convention: if so, it is possible the *motto* to the work may have struck his notice. It runs as follows:

“If you would make a book, or write one,
Or get some body to indite one,
Don't think, because 'tis understood
By men of sense, 'tis therefore good;
But let your words so well be plann'd,
That Blockheads can't misunderstand!!”

The error we have pointed out is a gross and unpardonable one. We trust, that no member of the convention will, in any heedless mood, be led to prescribe, or take himself, the one for the other *aqua potassæ*!

We would be glad to know how, according to chemical language of the present day, the *so called, muriat of gold*, (p. 89,) deserves the title? Is it not a triple salt—a *soda-muriat* of gold? The time was, a man's heart was on his left side, but the colleges now order it differently! .

Before concluding, may we ask, whether we are to take the Latin or the English prescription in making the *confection of senna*, (p. 107)—because, the proportions of the ingredients differ in the two! This we shall ascribe to a typographical error: and yet, so long preparing for the press, and so long in printing, this work might reasonably have been expected to be perfect in this particular.

The name of *decoctum scillæ*, seems a very inappropriate one, since the squill is only in amount *three drachms*; whilst juniper is four *ounces*, and *seneca* three ounces; or, in other words, it is only about the eighteenth part of the others!

We must confess, we regret to see so many EXTRACTS introduced. There is scarcely one, the active part of which is not better procured in powder simply: for, independently of the ready deterioration of the article in the form of extract, so as rarely to be depended on—it is often spoiled from inattention in the preparation itself. Amongst them we find, (shall we say, *though last, not least*,) the *extractum sambuci*! Is not this *very powerful remedy*, the old *rob sambuci*, or *quiddony* of elder-berries, under a new name? We suspect we may say of it, as of some others in the work,

“Begar—Here’s Monsieur Tonson come again.”

INFUSIONS, we have presented to us in sufficient number to satisfy the most fastidious practitioner! *almost two dozen*! good, bad, and indifferent! Some of them better for extemporaneous prescription, than for the permanent standards of a drug store. The same remark will apply, perhaps, to the *mixtures* for the most part.

With respect to the distilled oils, we would be glad to know, of the fourteen presented to us, how many are actually employed by the practitioner? We cannot well cull from them more than half a dozen.

We will ask the convention to prepare for inspection, exactly by their own formula, the pills of sub-carbonat of soda.

What is the exact difference (in the opinion of the convention,) between the *carbonat of potass*, (p. 191,) and the *sub-carbonat* of potass, (p. 193.) We presume there is some, as we have two distinct formulæ! And we may ask whether the name of *salt of tartar*, as connected with the last, is not more applicable to the former.

As we are not directed how to distil nitric acid, it would have been proper for the convention to have specified the salt left, which is to be employed in forming sulphat of potass.

What is the *super carbonat* of potass? (p. 195.) Do the

convention seriously believe the salt produced in this process, is a *super carbonat*? Does it possess any property of a *super-salt*? Of what use is the formula, for *dried muriat* of soda?

Fifteen SYRUPS! a goodly number! among which we find, *syrup of sarsaparilla*!! and *syrup of sarsaparilla* and *guaia-cum*!! Also syrup of balsam of Tolu! in which to *two pints* of simple syrup is added one ounce of tincture of Tolu—or about *half a drachm of Tolu* in the two pints of syrup! a most powerful means of cure!

We have presented to us, we think, *fifty-two* TINCTURES! one for each week in the year. How many will be employed by any individual! We have, at least, the benefit of choice. It is to be regretted, that *primary* and *secondary* lists, had not been formed of this squadron. With such a host to guard us, in what shape can disease possibly assail us? excepting that of a *dram*!!

If the convention had given us only three more *ointments*, we might have had the pleasure of denominating one after each of the United States.* As a novelty in nomenclature, there might have been some merit in this, as connected with a new Pharmacopœia. Is it not probable, seriously speaking, that a smaller number would have answered every purpose?

We regret, in terminating these hasty remarks, to say, that a particular inspection of the whole work has led us to the conviction, that the old adage, of *multum in parvo*, is here reversed. How our good friends in Europe will receive this scientific production, time will show. Our observations are not made from any desire to depreciate the work in question: we wish to point out those errors, which we deem to be present in it. If in any instance we have incorrectly judged, we shall with pleasure acknowledge we were wrong—but here we must apply the letters of the mathematician, Q. E. D.

Having thus given a cursory view of different parts of this work, we shall, before concluding, advert to the

* There is certainly as much real merit in such a plan of nomenclature as in the old one, which denominated an ointment the *Unguentum Apostolorum*, *Egyptiacum*, *Neapolitanum*, &c.

propriety of publishing a work of science in both the Latin and vernacular language. On this point, a difference of opinion will probably prevail. The reasons assigned by the convention, are by no means satisfactory to us. In all countries where works of this description have emanated from authorized bodies, they have been satisfied to print it in the language of science—although they may also have sanctioned its *separate* appearance, in the language of the country. By this measure, every one purchased *that edition* which was useful to his wants—but here we are clogged with a double expense, which seems only intended for the benefit of the publisher. In the present state of medicine in this country, it unfortunately happens, that neither physician nor apothecary is *necessarily* obliged to possess an acquaintance with the Latin—and, since the fact is, that too many of both professions, are absolutely ignorant of even its first principles; it would seem most proper to dress this work in an English costume. There is, however, some reason to fear, that this facility would prove an obstacle to the desirable event, that those who devote themselves to either pursuit, should be compelled to have an acquaintance with that language in which some of the best treatises on medicine and pharmacy have appeared. Under this impression, we confess we regret its not being confined to the Latin. At all events, *one* of them is altogether superfluous—nor will many persons approve of the additional expense thus incurred. It is, in fine, questionable, whether it will not impede the general extension of a work professedly belonging to the nation.

It may be affirmed, that the reasons assigned in the preface of the work for publishing in both languages, are equally conclusive, as to the necessity of giving both the Historical Introduction and Preface, in a Latin as well as in an English dress. If the nomenclature and all the essential parts, required both languages, in order to be rendered “more intelligible to foreigners,” &c. certainly that part which is explanatory of the general design, ought likewise to have been rendered equally easy of comprehension to

them ! It may, perhaps, be said, that they could readily obtain a translation of the part—but the observation will apply as forcibly to the other, and hence we cannot help admitting the imperfection of the work, in this particular.

We have now only further to inquire, by what authority an *unincorporated* set of men, meeting upon business of general tendency, could exceed the bounds of their appointment, by selling the copyright of a work, to which no individual in the nation can lay exclusive claim. It is certain, none of the members of the convention separately, can maintain a right to hold the copy of this work as his own—and it would be difficult to show how they could, collectively, acquire such a right, without a power ceded to them by their constituents. This does not appear to have been the case—and, indeed, it might have been supposed, that the honour thus conferred upon them by their medical brethren, would have amply repaid them for individual expenses. Even admitting the right to exist, it could not be deemed to extend beyond that period, at which all the expenses of preliminary conventions, and of printing, &c. should have been reimbursed. As it is, the copyright exists for fourteen years: *but for what ?* for a work, which will probably require immediate revision, and which, when effected, certainly cannot belong to the present proprietor. We must, therefore, protest against this measure, in behalf of every practitioner of the Union—especially as it will be found, that of all the contents, not a twentieth part is absolutely new, but is derived from the European pharmacopœias, &c. to which every member of the community has a right to recur, without infringement on the work before us.*

We recommend to every physician and apothecary in the United States, to consider well the merits of a work, which

* We should not have noticed this subject at all, had it not been for the *presuming* notice of the proprietors of the Pharmacopœia, that "*they shall be under the necessity of promptly instituting a suit against any person or persons who may attempt an infraction of their rights,*" &c. and cautioning all persons, whom it may concern, to govern themselves accordingly. We know one person, at least, who will venture to maintain *his own* rights as a member of the profession; and who would cheerfully meet them in a court of justice, if only to expose the inadequacy of the present performance.

professes to claim the high rank of Pharmacopœia of the United States. It is essential to the character of the nation at large ! it is more peculiarly so to the medical profession, that its real value be properly appreciated ; since we may assert with truth, that every person must be interested in a book which may be said to contain the issues of life and death. If, upon thoroughly investigating its contents, it be approved of, let it universally be upheld : if defects are observed, let them, by the call of another convention, be speedily rectified : and in the meantime, let the members of the last convention, be required most assiduously to buy up the whole impression, that not a copy may reach the European shores. If this is not done, we here protest, on behalf of our medical brethren throughout the Union, against the misnomer by which this work is denominated, the Pharmacopœia of the United States, and will cheerfully unite in any proper measure, by which it may be speedily set aside.

Among many others, which probably exist, the following errors, we hope of the press, have been detected.

Page 66. *Alcoholis.*

72. *Acetas liquidus.*—This is called *liquidus* in the Index, p. 267.

74. *Unciis quatuor.*

Unciis octo.

Quere—If *singulis octantibus* be correct Latin, p. 84.

Page 86. *Arseniatis.*

90. *Fluidunciam tres.*

104. *Scammonia.*—also p. 180.
minima viginti, for fluid scruple.

106. *Uncias quatuor*, and *uncias tres*—misplaced.

108. *Acetas præparatum.*

132. *Limaturæ Immegetetur.*

135. *Muriat of ammonia.*

156. *Ammonii*, for *antimonii*.

170. *Ol. cunilæ*—no such article on the lists.

Ol. Gaultheriæ—from a secondary article.

Ol. monardæ, do.

- Page 202. *Viginti quatuor*—for twenty hours.
Biduum—for twenty-four hours.
 220. *Alcoholis diluti*—for alcohol.
 224. *Pulverem*, for coarse powder.
 226. *Santalini*. 232.
 232. *Caryophylli*.
 240. *Cari*.
 252. *Liquefac*—for mix.
 258. *Uncias duos*.
 272.—Index. *Tabacco*.
-

Art. XXI. *Traité de la Fievre Jaune*. Par JEAN DEVEZE,
 Docteur de la Faculté de Médecine de Paris, Médecin du
 Château des Tuileries, &c. &c. Paris. 8vo. pp. 311. 1820.

THIS treatise is divided into three books. The first contains all matters of fact and personal experience: it is principally filled with the history of the yellow pestilence of Philadelphia during 1793, and of Cadiz in 1800. We are gratified to find that these topics, as also the description of the disease, and its effects on the human body, before and after death, bear satisfactory testimony of authenticity and correct observation. Few medical writers, indeed, have been so long and so often professionally engaged in the task of relieving the sick and checking the progress of a widely spreading pestilence in so many parts of the world. He is, therefore, emphatically entitled to the privilege of advising, on the subject, the government of his own country and the ruling powers of other nations. Were we called upon to decide conscientiously on the qualifications necessary for a good judge in these matters, we would say, that as they are of a nature which permits not speculative science to overreach the boundaries of experience and observation, a man should be pointed out who, with a rare and liberal composure of mind, has been tried amidst conflicting parties and systems, during a long professional career. It is such a one who can best bid defiance to sceptical theories, and establish the most probable and rational inferences.

The second book relates to the causes of yellow fever—of *infection* and *contagion* in general—and treats of the laws governing diseases from either source, of one or the other character, so as to determine the class to which this pestilence must evidently appertain.

This great question, which seems to have been decided by the majority of professional judges in this country, still remains a great mystery among the European governments. Their fear on the subject is the more surprising and lamentable, as they are not aware that from time immemorial, yellow fever has been an *endemic* of the West India islands, and the tropical continent of America, and never has shown itself as a remarkable and destructive epidemic in the northern ports of Europe or the New World. We may, besides, mark a period of forty years, at least, from before and after the American Revolution, during which time an unrestrained intercourse of large armies and naval forces took place between the great nations of Europe, and the occidental colonies, unaccompanied with pestilence. Their anxiety, therefore, to trammel their foreign commerce and intercourse with quarantine restrictions, may be justly attributed to the long and pertinacious attempts which have been made since the lapse of the last century, to establish the doctrine of a specific and contagious attribute in malignant epidemic fevers of various forms. An increasing mass of evidence is, however, daily produced against it, not only in this country, but among those European nations which have the most strenuously given name and substance to an imaginary *virus*. We must, for the present, omit to mention the honourable list of champions in the contest, to arrive at the system of our author, which deserves to be summed up in a correct syllabus.

1st. There are *occasional* or *accidental* causes of yellow fever. The *necessary* ones are those, without which it never prevails as an epidemic. These are two: *atmospheric heat*, and a *seat or centre of putrid fermentation*, rendering the air infectious, which is then denominated a “foyer

d'infection"—that is, a fomes or a vehicle of infection. The first characteristic is admitted by all theorists—and it matters not that its quantity or modification with other agents, should chemically remain undefined. The second is so far clearly understood and admitted by both parties, that neither can understand from it any thing else than *infection* or *contagion*. We are also all agreed that *intermittent fevers*, *remittent bilious fevers*, *dysentery*, *jail* and *hospital fevers*, *typhus*, &c. can be traced to a source of infection.

2dly, That they attack a great number of people at once, who have no previous communication with each other, and none of whom were previously affected with the disease.

3dly, That they are all singly susceptible of transformation from the shape of one into that of another, and they consequently belong to one and the same class of diseases.

4thly, That they are exposed to the operation of heat, winds, and waters, and become by those causes more or less aggravated.

If these characteristics constitute the class of diseases from *infection*, in relation to their formation and occurrence, there are others no less comprobatory of the same origin, inasmuch as they are of the same nature, however various in their forms and degrees of intensity. Thus the above named diseases may be produced,

1st, By one and the same kind of infecting atmosphere.

2dly, They will succeed to each other, according to seasons, or be inverted with their vicissitudes. The spring brings on intermittents in the vicinity of ponds—summer, bilious remittents—autumn, dysentery and typhus, &c.

3dly, A succession of these diseases may take place in one individual constantly exposed to miasmatic influence: he may have his intermittent, remittent, typhus, &c.

4thly, In most complaints thus contracted from infection, many symptoms are alike—such as irregular chills, disordered *primæ viæ*, frontal pains, critical sweats, &c. The same organic lesions are also discovered by autopsic examination.

It remains now with the author to find another series of approximating circumstances, which may explain the different results from the same cause or source of infection, and to show that the quantity and continued operation of deleterious particles must excite malignant diseases, though not so dissimilar from those we have noticed, as that they may not all be classed together. He therefore proposes to consider,

1st, The extent or intensity of the centre of putrid fermentation.

2dly, The distance or proximity of the exposed individuals.

3dly, The period of the season.

4thly, The movements of the atmosphere upon the sources of infection.

We conclude, therefore, that with the concurrence of atmospheric heat upon a seat or centre of infection, many diseases are generated with characteristics constituting a *class of diseases from infection*—that if the latter operate with aggravating circumstances, the result will be *malignant fever*, which, under different shapes, may still be assimilated to other diseases, all appertaining to the class of those that are created by infection.

The second part of his argument tends to define and compare *contagion*—the attribute of which is its own reproduction.

1st, It is communicable by contact, or by some other indirect means.

2dly, It cannot be accounted for *by any known cause*, except that of communication.

With these unerring data, it has been necessary to designate the matter or substance constituting a contagious attribute by the name of *virus*. It is chemically undefined and unknown in its constituent principles, unless by its uniform effects, which are,

1stly, To generate a malady perfectly similar to that which has produced it.

2dly, To preserve for a time the power of re-creating the same.

Besides these characteristics which unexceptionably appertain to all sorts of *virus*, each kind has other properties.

Could we ascribe the yellow fever to the above class of contagion, or contagious diseases, we should find a sufficient number of characters or signs belonging to the latter by perceiving the circumstances of its history,

1st, In its formation.

2d, In its propagation.

3dly, in its existence (*état*) or duration.

4thly, In its disappearance.

But none is found in the period of *formation*, because this requires, as stated, two *necessary* conditions, that of high temperature, and the second a centre of fermenting putrefaction, or other local sources of effluvia elicited from corruptible materials, neither of which is demanded for the formation of contagion in any climate or season, under the equator, or under the polar regions. An abundant collection of facts is produced in support of this proposition.

The propagation of yellow fever exhibits also its absolute difference from contagious diseases. Interrupted in its march, it irregularly invades a large city. It does not irradiate from a centre to the circumference, and has been known, after long pauses, to be renewed with the greatest fury, and never to show, throughout its course, that its diffusion was aided by contact with persons—but only by exercise abroad, by night air, and by the residence in, or the visiting of, unhealthy places or districts. In its duration, yellow fever presents still more singular and astonishing phenomena, which cannot be explained by supposing that it exists under the sway of contagion. Besides, its frequent interruptions, its disappearance from districts where it was raging, and its bursting out where it is the least expected,—it varies in its symptoms, types, and intensity. It has been remarked to be rapidly mortal in its commencement, and towards the close of the warm weather, when the vapours of the earth do not ascend high in the atmosphere. At

other times it proves very mild: in airy places and hospitals it is frequently controlled by remedies which are abortive in the city—and thus many physicians have not been in want of facts to show the success of a variety of specifics, in which others could not discover the least efficacy. The epidemic also has never been known to outstrip its boundaries, and pass into a neighbouring village or island,—and is observed to exist in all possible shapes of fever and ague, of remittent, of dysentery, of continued fever, &c. &c. Is there, in these features, any thing that characterises or denotes a contagious disease? Could such a disease exhibit, under the influence of winds, heat, rain, as material changes and variations? In the last stage, the yellow fever recedes in an inverse direction to that by which it was diffused. The spot in which it commenced and raged the most, is invariably the last which it abandons. Whatever vicissitudes, however, of the atmosphere may have mitigated its progress, there is only one, at best, which puts an end to its renovation. This is the degree of cold which checks exhalations, and purifies the surrounding air. But even then, situations in the vicinity of a centre of putrefaction are dangerous, since, should temporarily warm weather return, the disease will be renewed with the greatest malignity.

The author has been aware in his dissertations of having enumerated some circumstances which may be applied by syllogistic reasoning to epidemic contagious diseases,—and he has granted them all—being satisfied that a few exceptions can never invalidate the force of his argument. He concludes, nevertheless, by additional inferences, to prove that yellow fever must appertain to the class of diseases by infection.

1st, It is the *maximum* of diseases of that nature.

2d, Every one of them is the effect of deleterious substances on the animal economy.

3d, Black vomit and jaundice are, in a less degree, observed in other infectious fevers.

4th, Mild cases of the yellow fever are frequently, by the

mode of treatment, resolved into the symptoms of ordinary fever, &c.

The succeeding chapters of this part of the treatise, contain various points of general interest—accounts of importations, in modern times, of the disease, into several large cities of Italy and Spain—where, as in America, the allegation has not failed to be credited by fear and misrepresentation. He resumes, also, the most plausible arguments of the contagionists, and dispassionately discusses them with the utmost propriety. One of them, is the coincidence of the importation of the *exotic virus*, in ships and goods, with the sudden or gradual formation of the epidemic,—from which it is agreed, that admitting all plausible causes of domestic origin, it must, however, be inferred, that yellow fever is sometimes contagious. His answer to this, from page 211, we will give in a free translation.

“After what has been said of the laws governing the infection of malignant fevers, it would warrant no conclusion, to prove contagion by instances of individuals sickening, who approach those already infected, unless it is satisfactorily shown that the former had not been, or were not actually exposed to the causes which must create the fever by infection. If, however, that cannot be proved, there is no reason for us to reject a theory of the generation of the disease which is natural and well explained, in order to adopt one that is a phenomenon, neither understood, nor standing upon philosophical or physiological ground. It is illogical to oppose a proposition by a supposition. In the present instance, therefore, it remains with our antagonists to prove communicability beyond, and far from any center of putrefaction at *foyer d'infection*. Can they do that? We will attempt it for them, and the reverse will be evidently shown.” Here follows, among others, the narrative of an event very little known in this country. It is, the arrival in Philadelphia, at the most critical period of the prevalence of yellow fever in 1793, of a ship loaded with all the sick of the hospital at Cape François, preserved from the flames of that city. No strangers could be less acceptable, during

this general calamity, and the ship was ordered down to the mouth of the Schuylkill.

But as they were in the utmost distress, in want of every thing, many also being shockingly wounded, they, through the influence of the French ambassador and our author, were allowed to come secretly up that river, behind the city, as far as Bush-Hill, where barracks were erected for them around the hospital. They were gradually received into the wards and beds, as the yellow fever patients died off. The health committee then sold to the French, for the use of their soldiers who took refuge in Philadelphia, all the furniture and utensils of the hospital, and in the hurry of the change of owners, there was no time for the necessary purification and cleansing. Yet none of these occupants took the yellow fever. No such thing exists, therefore, as its *contagion*, since the disease cannot be elicited beyond a *foyer d'infection*.

The book concludes with the treatment of the yellow fever. It is a plain collection of valuable documents, which every practitioner may consult with advantage. It is in no way prejudiced towards any method, or favourite remedy, beyond what a sufficient experience and observation have fully warranted. One important inference from it, we beg leave to recommend to the consideration of the most zealous members of the boards of health of our cities. As they exercise prohibitory powers against every possible means of importing the supposed *exotic virus*, they are frequently induced to reprobate the principles and opinions of the contagionists, as dangerous to the community. "Perishable goods," says our author, (p. 301,) "long kept in a stagnant air, are always damp, and may be damaged—and hence they surely may be considered as *centres of putrefaction*, and very dangerous to those who approach them, until they have been perfectly aired."

Whatever decided opinion we may entertain respecting the prominent points of doctrine in the treatise before us, we do not feel disposed to let it pass without pointing out its imperfections. In a work, written in a foreign language,

we confess we have little to say respecting the style and the method of the writer. Yet, that in this case, the latter must be defective, we infer from the difficulty we have experienced in assembling all his argumentative, but disconnected materials.

Though, perhaps, his theory of *infection* and *contagion* is better developed for the purpose of comparative and practical application, than that of any anterior writer, it has been already indicated in the works of some contemporary investigators, to whom we regret that the author has not given due credit. We acknowledge that he has an exclusive right to the system which he alone has regularly formed. By the help of the arrangement he has adopted, we are enabled to perceive, that he has assigned to infection, (*foyer d'infection*,) a somewhat exclusive agency in the formation of malignant epidemic diseases, explaining, by the *elevation*, *continuation*, and *suddenness* of heat, either the nullity of its effects, when it should necessarily take place, or the insusceptibility of individuals, who by their idiosyncrasy, escape from it, (p. 207.) But these reasons are not sufficient, we think, to account for the long intervals of healthy periods which have often elapsed in the most sickly countries. Aware of the force of this objection, he answers, by many plausible observations, and re-intrenches himself at last in the belief, that for many years, malignant disease may have existed without ever having been noticed or attended to. We hope he is too wise to insist upon such an inference, and to remove all difficulties, we invite him to admit the concurrence of *atmospheric constitution*, and thereby complete his theory of infection.

Sunt aliquot quoque res quarum unam discere causam non satis est.

De Nat. Rer. Lucret. lib. vi.

The meaning of atmospheric constitution, would have been as undefinable to Hippocrates or Sydenham, as it is to us. The late Dr. Rush considered its influence as distinct from the local causes of diseases. Its changes, during seasons, and in different climates, has always been the theme of medical philosophers, who attributed to its combined

agency, the production of epidemic diseases, and of some also, which existed during remote ages, and are no more to be seen, as well as others among us, which were unknown to former generations. We are not able to explain by what law or planetary revolution, the lower orbit of the earth becomes unpropitious to animals. We only know, that the atmospheric constitution has been sometimes deleterious during long periods of years: it was such all over Europe and Asia, under the reigns of the two Justinians, in the sixth and eighth centuries. There have been again, long periods of universal pestilence, during the fourteenth, fifteenth, and seventeenth centuries. The black pestilence of London, is referred to the first. Those of Stockholm, Nimeguen, Dantzic, Hamburg and Moscow, prove evidently, that the operation of continued heat is not always paramount to that of the atmospheric constitution. Long before and after the plague of London, in 1665, meridional Europe, from Paris to Rome, was frequently the theatre of malignant catarrhs, influenzas, and sore throats of different kinds, and as fatal as the plague, until the memorable destruction of human beings by pestilence at Marseilles, in 1721. We want data to determine at what degree atmospheric agency can be more deleterious than local causes of infection, unless it be the winds which spread its influence.

It is said that Empedocles, of old, saved his country from a grievous mortality, by filling up the cleft of a mountain, and thus intercepting a pestiferous blast which blew through it. The winds of Africa frequently destroy the caravans crossing over the desert. Contrary to these destructive phenomena from above, we are informed, that in many tropical districts of South America, no animal or vegetable putrid effluvia can corrupt the air, so as to endanger the health of the inhabitants. With more instructive researches than we can at present collect in a desultory point of view, it would be easy to prove, that a certain *atmospheric constitution* has always concurred with local causes, in proportion to the intensity of those pestilential scourges which have threatened the entire annihilation of men and beasts.

These remarks may serve to indicate, that besides the mysteries which have been already explored by patient labour, there are others yet left to the genius and industry of philanthropists and the learned.

In the spirit of perfect good will, we invite our author to correct his note, (p. 258,) where he improperly accounts for Dr. Rush's silence on the subject of his *dissertation*, by a prevailing pride of the writers on this side of the Atlantic, inherited from their British ancestors, and by which they consider it disgraceful to appear indebted to foreigners. Whatever may be the antipathies felt by the two great neighbouring nations of Europe, they cannot thrive here, where there is no national rivalry, nor contention for power. We may add also, that the writings of Rush are numerous enough to bear evidence against his possessing national prejudices, especially in scientific matters. Nor could envy inhabit the bosom of a man, who had placed his fame even above the obloquy of his own errors.*

ART. XXII. *A System of Practical Nosology: to which is prefixed, a Synopsis of the Systems of Sauvages, Linnæus, Vogel, Sagar, Macbride, Cullen, Darwin, Crichton, Pinel, Parr, Swediaur, Young, and Good. With References to the best Authors on each Disease. "Auctorem neminem unum sequar; sed ut quemque vicissimum in qua parte arbitrabor."* *Plin. Hist. Nat. lib. iii.* By DAVID HOSACK, M. D. F. R. S. L. & E. Professor of the Institutes and Practice of Medicine in the University of the State of New-York, one of the Physicians of the New-York Hospital, &c. Second Edition, corrected and enlarged. Printed for the Use of the Class. New-York. 8vo. pp. 386. 1821.

NOSOLOGY, in its various relations, has engaged much attention, and been discussed with no ordinary ability, within the last thirty years, by the medical men of the

* To one of the most distinguished of our medical men, we are indebted for the above acute and faithful exposition of a work, which has attracted no less attention from the authority of the writer, than the profound interest of the subject.

United States. That some classification of diseases is required by practical convenience, independent of other considerations, is sufficiently admitted by the parties in the controversy. But, as regards the precise mode of arrangement, there is still a wide, and, perhaps, an irreconcilable difference of views.

Ever since the time of Sydenham, the custom has prevailed, with one or two exceptions, to group them together pretty much on the principle adopted in relation to the objects of the strictly natural sciences. We have neither time nor inclination to give any account of the various schemes of this description, which have been obtruded upon us. To the work under revision, and more particularly, to Good's *Treatise on Nosology*, we refer such as are desirous of detailed information on the subject. Each of the many systems to which we have alluded, is possessed of some share of merit, though the whole are replete with very gross and obvious defects.

Aware of the miscarriage of all preceding attempts to assort or arrange diseases, the celebrated Brown, in the bold spirit of innovation which so conspicuously belonged to him, made a vehement denunciation of the nature and tendency of nosology.* The light in which he contemplated diseases, was marked by extreme simplicity. No other distinction could he perceive, or rather did he recognize, in morbid affections, than as related to intensity of excitement. Diseases, therefore, he brought under the general heads of *sthenick* and *asthenick*, or those above and below the standard of health.

Notwithstanding the credit so generally accorded to him for the originality of this arrangement, he seems, in this respect, to have very slender pretensions. To the medical methodists of antiquity, who maintained that diseases chiefly depend on too great rigidity or laxity of fibre, he was unquestionably indebted for the primary suggestion of this scheme. The same notion may be traced even to a much

* "Nosologia delenda est!" is a memorable expression that escaped him, denoting emphatically the warmth and tone of his prejudices.

remoter date. Coincidences of this sort are indeed often very curious. The initial paragraph of Aristotle's first problem, contains the following interrogatory: "Why," says he, "have great excesses a tendency to produce disease, but because they occasion *too much or too little excitement, in which all diseases consist!*"*

By our own school, an example has been afforded of still more intrepid generalization in this case. Denying altogether the diversified nature of disease, it became necessarily part of the same system to put down, completely, nosological arrangements, as utterly absurd, as well as highly pernicious, in their practical applications. But this, like most other denunciations, was, perhaps, urged intemperately. No one discerns more clearly than ourselves, the defects of the existing plans of nosology. Candour, however, compels us to confess, that even the worst of them is not wholly without use, since it introduces into the complex science of disease, to a certain extent, at least, order and perspicuity. What was said of his favourite science by Linnæus, is equally pertinent to medicine. "*Filum Ariadneum Botanices est systema sine quo chaos est res herbaria.*"

It is not fair, in reasoning on nosology, to deduce our objections from its abuses. This, at all times a very dangerous mode of argument, and by pursuing which, even the most sacred subjects may be rendered ridiculous, is more particularly so as relates to classifications. Every arrangement of natural objects, with which we are conversant, is amenable to such criticism, and their imperfections have often indeed been exposed and derided.

Can any thing, for instance, be more incongruous or absurd, in any system of nosology, than the placing, as has been done by Linnæus, in the same class, the human being, and the common bat,—the first, and nearly the last link in the chain of animal existence. But who, on account of

* We take the passage from Webster on Pestilence, vol. i. 221:—the work of Aristotle we have no opportunity of consulting.

this most unhappy collocation, denies the general utility of zoological classifications, or even the particular arrangement of this illustrious naturalist?

As to the best mode of systematizing diseases, it is not easy to determine. But, that some is absolutely required, seems, as we have already intimated, to be indisputable. Even those who are loudest in their condemnations of nosology, have tacitly conceded its necessity by adopting its nomenclature and its distinctions. This was the case with Brown, and surely it has not been less illustrated in the conduct of those who more recently opposed it.

To dispose diseases, according to their affinities, into classes, genera, orders, species, and varieties, as has been so frequently done, strikes us to be artificial, and probably impossible, in the present state of our knowledge, from the difficulty of settling, with any kind of precision, the degree of consanguinity, or relationship, which may exist among different morbid affections.

It early occurred to us, that they might be arranged as they presented themselves in the several parts or systems of the body. That this plan has its defects, we shall not deny—though, on the whole, it seems to be the most natural, simple, and practically useful. Limiting the term system to a part, or combination of parts, which have a similarity of structure, and concur in the performance of the same offices, the following, we think, may be made, without any unnecessary minuteness of division.

1. *The circulatory*, consisting of the heart and blood-vessels.
2. *The digestive*, consisting mainly of the stomach and intestines.
3. *The absorbent*, consisting of the lacteals and lymphatics.
4. *The respiratory*, consisting of the pulmonary organs.
5. *The secretory*, consisting of the large glands.
6. *The sensitive*, consisting of the organs of sense, of the nerves, the brain, and spinal marrow.
7. *The muscular*, consisting of the muscles, with their tendons, aponeuroses, &c. &c.

8. *The cutaneous*, consisting of the external covering of the body.

9. *The osseous*, consisting of the bones and their immediate appendages.

10. *The generative*, consisting of the genital apparatus in both sexes.

To comprehend, under these heads, with sufficient perspicuity, all the diseases to which the human frame is liable, has proved to us a task of no great difficulty or embarrassment.* We treat of them, however, without any very particular reference to their alliance or affinity. As previously remarked, the decision of this point, in many instances, would be exceedingly perplexing, and in a practical view, is hardly worth the trouble of adjusting. Yet while we thus reject the artificial division of diseases, for such divisions, till we become more enlightened in pathology, must, to a certain extent, be artificial, we inviolably retain the names as established. These are expressive, and being every where received, constitute, at present, the language of medicine, as sanctioned by general approbation, and consecrated, as it were, by universal usage. To change, is always more or less an evil, and we are not sensible, that in many cases it is productive of greater inconvenience, than in the techniology of the sciences.

In our general scheme, as well as some minor points, incidently mentioned, we have the misfortune to differ from the distinguished author of the work under examination. He, in the construction of his nosological system, adheres to the ordinary rules of classification, distributing diseases into eight classes, which are again subdivided into orders, according to the supposed resemblances, and requiring the same general principles of treatment. The classes are,

- | | |
|----------------|-------------------|
| 1. Febres. | 5. Suppressiones. |
| 2. Phlegmasiæ. | 6. Neuroses. |
| 3. Cutanei. | 7. Cachexiæ. |
| 4. Profluvia. | 8. Locales. |

* This is the arrangement adopted in the lectures on the practice of physic in the University of Pennsylvania.

Of the subordinate divisions or orders, we have not space to allow of any further notice.

Did we feel disposed to indulge in any minute or formal criticism, we might, perhaps, expose some incongruities in this assortment of diseases. But we are aware, that no former attempt is exempt from similar faults, and which we believe, indeed, are inseparable from all such undertakings.

We beg leave merely to submit, for the consideration of the author, whether his plan might not be improved by removing dysentery from the class *Febres*, to that of *Phlegmasiæ*, and in taking *satyriasis*, *nymphomania* and *apoplexy* out of the *Neuroses*, and locating them in one or the other of his first classes. Certainly, they cannot be nervous affections, which are cured only by depletory measures, active and continued. Nor do we acquiesce in the propriety of placing *plethora* among the *Cachexiæ*!

These, however, are blemishes which are more than redeemed by the general merits of the work. As a system of *Nosology*, it is in our opinion, decidedly superior to any on a similar plan hitherto published. It is marked by greater simplicity of classification, by a nicer discrimination of disease, and by a more close and faithful adherence to nature, and has none of the *outrageous* collocations, which disfigure every preceding attempt. The utility of the work is enhanced, by a synoptical view which is prefixed, of the different arrangements of disease, from the time of *Sauvages*, to that of *Young*, inclusive, and still more, we think, by a copious catalogue of the best writers on each disease, appended as notes. In the execution of this part of his design, our author displays vast research, an intimate acquaintance with books, and fully vindicates, in every respect, his titles to literary skill, and medical erudition.

We should not take leave of a work which, on the whole, we estimate so highly, without pressing it more strongly on the attention of our readers, were we not persuaded, from the rapidity with which it has passed to a second edition, that

they are already pretty well apprized of its value, and, as a native production, of its paramount claims to notice.

ART. XXIII. *Experiments on the Organs of Absorption*. By JAMES SOMERVILLE, M. D. An Inaugural Thesis. University of Pennsylvania. 1821.

THE late Commencement in the University of Pennsylvania was more than usually distinguished by the number and excellence of experimental theses, submitted by the candidates for graduation. Of the best of these we intend to give some account, as we may find room for the purpose.

It is pretty generally known, that the hypothesis which attributes an absorbent power to the minute extremities of the veins, has been, within a few years, so plausibly supported by Mr. Magendie, that it would require, with some, very numerous and well conducted experiments to refute his particular views. Yet we are by no means prepared to admit his conclusions, nor are we convinced that his experiments, in the hands of persons of cooler temperaments, and less addicted to hypothesis, would be attended with the same results. The author, indeed, of the thesis before us, has obtained from his experiments, results so diametrically opposed to those of Mr. Magendie, that they must, in a great measure, impair our confidence in the deductions of this indefatigable physiologist.

Though our author has examined the general question of venous absorption, his attention and experiments have been more particularly directed to that part of the subject, which concerns the action of poisons applied to wounded surfaces—and to this point we shall at present limit our observations.

The effects of a poison on the system, under such circumstances, can only be explained by one of the following suppositions: 1st, that it enters the circulation through the divided vessels; 2d, that it produces its effects through the

medium of the nerves; or 3d, That it is absorbed by the lymphatics. The first of these opinions has been supported by the well known labours of Mr. Magendie, and Mr. Brodie, whose numerous experiments are, by many, considered as conclusive. These, however, the author of the Thesis has endeavoured to invalidate—with what success, we will proceed to examine.

The first of his experiments, is the repetition of one, upon which Mr. Magendie lays much stress. The results, however, obtained by the two experimenters, are widely discordant.

“In experiment first, the hind leg of a dog was amputated, leaving only the crural vein and artery. An infusion of *nux vomica* was put into an incision made in the amputated portion of the limb. After this had remained four or five minutes, without producing any sensible effect, a leaf of tobacco, which had been moistened in warm water, was applied to the incision. This, together with the *nux vomica*, remained half an hour, during which time the circulation through the crural vessels of the amputated limb continued quite vigorous, without the least sensible effect on the dog.

“That we might ascertain whether this failure was owing to any defect in the poison used, we placed upon the stump of the amputated limb, a part of the same infusion of the *nux vomica*, and the leaf of tobacco. In five minutes the dog became affected with tremors and convulsions, and in fifty minutes he died.

“In the second experiment, the same process was repeated, except that in place of the *nux vomica*, a distilled preparation of tobacco was employed, which was suffered to remain upwards of half an hour, and no effect was produced by it. Of the same preparation, a portion was applied to the stump, which in fifteen minutes occasioned great prostration of muscular power, dilatation of the pupils, and considerable nausea. In this state, an infusion of *nux vomica* was applied to the stump, which proved wholly inert, owing probably to the destruction, by the tobacco, of that principle in the nerves, which causes sympathy, or consent of parts.”

By Mr. Magendie it was found, on the contrary, that the effects of the poison, (the upas,) in similar experiments, "were as quick and intense, as if the limb had not been at all separated from the body—the animal being influenced by the poison in four, and dead before ten minutes." To obviate the objection, that the coats of the vessels still contained lymphatics, his experiment was varied. He introduced into the crural artery, a small cylinder of a quill, to which he fixed the vessel by two ligatures; the artery was then divided circularly between the ligatures, and the crural vein was treated in the same manner. Thus, though there was no communication between the thigh and the rest of the body, except by the arterial and venous blood; the poison introduced into the foot, showed its effects as usual, in about four minutes. This experiment was repeated four times by our author, and not "the slightest pulsation," says he, "could we perceive in the artery below the quill, and in a short time, the limb became cold: on making an incision into it, we found the blood coagulated."

Having in vain attempted to convey the influence of the poison into the system, through the crural vessels, in the experiments just related, our author was, of course, unable to verify the curious circumstance mentioned by Mr. Magendie, that by pressure on the vein, the effects of the poison might be arrested. "To render the phenomenon more evident," says Mr. Magendie, "we compressed the vein between the fingers at the instant when the poison began to be apparent. They ceased immediately, and reappeared as soon as the compression was discontinued: they may thus be regulated at pleasure!"

Next Mr. Magendie's experiment is adverted to, in which the blood from the crural vein of a dog, into whose leg poison had been inserted, was transfused into the jugular vein of another animal of the same kind. The process was continued for five minutes, without producing any effects. Being then stopped, and the blood of the crural vein allowed to return to the dog to which it belonged, this animal almost instantly exhibited evident signs of the poison. This

experiment is thought strongly to militate against Mr. Magendie's statement, and so it seems to us—since if the poison is conveyed to the system through the veins, the blood containing it, could not prove fatal to one dog, and harmless to another, under circumstances equally favourable to the action of the poison !

But the doctrine espoused in the Thesis, is also opposed by the experiments of Mr. Brodie, who has investigated the action of poisons on the human system, apparently with great care and success. This distinguished physiologist is led by his inquiries to the conclusion, that poisons taken internally, operate through the medium of the nerves, without being absorbed into the circulation—whereas, those applied to a wound produce their effects by entering the circulation through the divided blood-vessels. The following are some of the experiments, from which he deduced the latter part of his conclusion.

“ I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane, so that the artery and vein were left entirely insulated. I then made two wounds in the forearm, and inserted into them some of the woorara, formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided.”*

To this experiment it is objected by our author, that the nervous communication is so minute, and so completely disseminated through the whole system, that it is impossible to destroy it, without the entire division of every medium of communication that is possessed of vitality.

“ I exposed,” says Mr. Brodie, “ the sciatic nerve of a rabbit, in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the

* Trans. Royal Soc. Lond. 1811.

woorara, I tied the tape moderately tight on the forepart of the thigh. Thus I interrupted the communication between the wound and the other parts of the body, by means of vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour, I removed the ligature: twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison.*

In two other instances, in which Mr. Brodie performed this experiment, the ligature was drawn very tight, and was removed, in one, after an hour and twenty minutes,—in the other, after three quarters of an hour. In neither case was the animal at all affected by the poison.

These experiments seem to prove, that a communication by means of nerves, is not alone sufficient to convey the influence of poisons to the system. "But," says our author, "by arresting the circulation, the vitality of the limb is destroyed," or at least, the nervous action is suspended, and the limb is rendered insusceptible of receiving the impressions of the poison applied to it. This argument is certainly sufficiently plausible to authorize us to refuse our assent to the inferences of Mr. Brodie, who considers his experiments as incontrovertibly proving, that it is not through the nervous system that the action of the poison is imparted.

The third opinion, namely, that poisons are absorbed by the lymphatics, and their effects thus communicated to the system, which was for a long time the prevailing one, seems now to be losing ground among the experimental physiologists of France particularly. Yet we are not aware that it has been disproved, or indeed, materially weakened. The experiment so often performed by Brodie, Magendie, and many others, and repeated by our author, in which, though the thoracic duct was tied, the poison affected the system, is allowed by all to be inconclusive. Even admitting the experiments of Mr. Magendie, referred to above, and also, that the poison is conveyed through the

* Trans. Royal Soc. Lond. 1811.

blood-vessels, how is it demonstrated that it is absorbed by the venous extremities, rather than by the lymphatics, which, according to Magendie, communicate freely with the arteries? The consideration of this question, however, would lead us into the controversy respecting the absorbent power of the veins, which, at present, we wish to avoid.

The Thesis concludes with the ensuing passage, which we extract, as exhibiting, in a condensed shape, some portion of the author's views.

"It appears to be beyond dispute, that the nerves have an important connexion with the operation of poisons on the system. The well known fact, that a solution of the ext. belladonnæ, when applied to the tunica conjunctiva of the eye, occasions a dilatation of the pupil, though no other part of the system is affected, proves that a poison may affect a distant organ, through the medium of the nerves, without entering the circulation. The effects of mineral poisons tend to confirm this opinion, for, in whatever way the poison is introduced, the inflammation is confined to the stomach and intestines, and it is commonly more violent and immediate, when these poisons are applied to a wound, than when they are taken internally, and it likewise precedes any appearance of inflammation of the wound. This fact was proved, by an experiment made by Mr. Hunter and Sir E. Home, and by many repetitions of it by Mr. Brodie. Upon the whole, we are led to the inference, that poisons act principally by their effects on the nervous system, and that this important part of our economy is the medium through which their deleterious effects are produced."

In closing these brief remarks, we feel it incumbent on us to confess, that the analysis we have given of the Thesis does great injustice to its merits. It is in every respect a most creditable production, containing much interesting matter, well put together, in a style of unpretending simplicity, and affords a pledge, which we hope soon to see redeemed, of performances, in this way, of still higher character.

The only apology, indeed, we can offer for so slight an exhibition of its contents, is the want of space, and that we shall have an early occasion to call again the attention of our readers to this subject, in the notice of a report of a committee, appointed by the most active of our medical societies, to repeat all the recent experiments relative to absorption.

OBITUARY.

COMMUNICATED.

DIED on Friday, December 8th, 1820, at his seat in the vicinity of Chester-Town, Maryland, in the 69th year of his age, Dr. JAMES M. ANDERSON. At an early age, having successfully terminated his academical studies, he commenced that of medicine under the direction of his father, a very respectable physician from Scotland. He attended a course of lectures by Professors Shippen and Morgan in the school of Philadelphia, then in its infancy, and next sailed for Edinburgh, at that time the focus of medical literature. Circumstances, which it is unnecessary to mention, not permitting him to remain long enough to obtain a degree, he returned to this country with an ample certificate, signed by his preceptors, Cullen, the elder Monro, and the whole board of Professors.

He commenced immediately on his return the practice of physic in conjunction with his father. Deeply versed in general, and particularly in medical science, and devoted almost beyond example to the performance of his professional duties, he soon obtained a reputation unknown to any of his competitors.

For a period of upwards of thirty years, he retained a practice of an extent certainly without a parallel in this section of the country. Advancing rapidly towards his 60th year, and feeling those infirmities consequent on a life so laborious, he retired to his seat near Chester-Town. In this situation, however, he was not allowed the repose which he anticipated. Though the native vigour of his constitution

was broken down by the invasion of disease, and by those accidents to which his course of life subjected him, he attended, almost to the close of it, to the calls of his patients.

As a physician, though attached to the doctrines of the old school, his rank was second to none in the State. Prompt in his decisions, and drawing from a rich fund of learning and experience, it may be truly said, that in his diagnostic discriminations and clinical calculations he seldom failed. As a husband, father, friend—in every domestic and social relation, the world had not his superior. His rank, as a Christian, was eminently distinguished. Equally removed from lukewarmness and enthusiasm, he was a disciple of Wesley, and strenuously contended, that not to admit the truth of his tenets, was to deny the obvious doctrines of the Holy Scriptures. Communicative and affable in his deportment to all, he never for a moment forgot the dignity of his character, or what it exacted. Easy of access, and acutely sensible to the wants of others, the needy sufferer rarely made a fruitless application for aid. His home was an asylum for the indigent, and such were his liberality and benevolence, that, though his practice was extensive and lucrative, he was precluded from the accumulation of wealth.

In his last and painful illness, his demeanour was instructive and exemplary. Patient to a degree seldom equalled, never surpassed, he was always thankful for the little attentions and services of his friends—and in the final trying scenes, submitted with meekness to the will of his Heavenly Father. His loss was mourned by all, and his death has made a chasm in our professional and social circles, which it will not be easy to fill.

TO READERS AND CORRESPONDENTS.

WE have received from Dr. Dean, a lengthened vindication of himself, against a supposed imputation on the part of Dr. Little, which appeared in the last number of this Journal, of his having assumed the credit of the original application of mercur-

rial ointment to the cure of erysipelas. The style of Dr. Dean is so courteous and well-bred, that, independently of other considerations, we could not hesitate, for a moment, to have inserted his defence, had it reached us in time. In reviewing the paper of Dr. Little, we confess, however, that we are unable to discern the alleged charge, or indeed, any thing else of an offensive nature.

The complaint of Dr. Little is, that the medical public, overlooking his claims, have awarded the credit of the remedy to Dr. Dean, and this he states without attaching to him any blame.

In justice to Dr. Dean, we are bound to mention, that though perhaps, not the first to use the ointment, he was in possession of the fact of its utility from Dr. Collins, of Kentucky, many years prior to the promulgation of it by Dr. Little.

We hope that this explanation will prove satisfactory to both parties. But should it not, the pages of this Journal are open to a well tempered discussion of the subject—and no other, we are sure, can take place between men, who have so many titles to reciprocity of respect, and motives to harmony and good will.

On account of its length, we could not find room for the valuable essay of our accomplished friend, Dr. Bell. It shall conspicuously appear in the ensuing number of the Journal.

To Dr. Godman, we are thankful for his interesting paper; and the same acknowledgment is due to Drs. Baxter, Perlee, Mitchell, and several others, for communications, of a very superior character, which have been thrown out for a time, by an absolute repletion of matter.

Professor Coxe has in the press, a new edition of the *American Dispensatory*.

M. Carey & Sons are printing, *Elements of Therapeutics and Materia Medica*. By N. Chapman, M. D. Second edition, enlarged and improved.

Also nearly ready for the press, a *Treatise on the Fevers of the United States*. By N. Chapman, M. D.

In this work, it is the object of the writer, in a style plain and didactic, to inculcate the principles applicable to the management of fevers, as modified by the peculiarities of our climate, and other causes—and in laying down the treatment of this class, he has endeavoured to embrace what is essentially necessary to the cure of most of the acute affections.

University of Pennsylvania.

The Medical Lectures will commence, as usual, on the first Monday in November next.

INDEX.

- ABSCESS**, lumbar, Dr. Horner's account of, i. 141.
Absorbents, Magendie and Brodie's experiments on, ii. 410.
Absorption, Somerville's Thesis on, ii. 408.
Academy of Medicine of Philadelphia, officers of, ii. 203.
Ague, quartan, cured by repeated emetics, ii. 334.
Alkalies, two new, recently discovered in Peruvian bark, by Messrs. Pelletier and Caventon, ii. 261.
Alveolar processes, on the devastation of the, ii. 282.
Amalgam, prescribed in a case of tænia, i. 136.
Anderson, Dr. James, obituary notice of, ii. 414.
Animal heat, on the generation of, ii. 250.
Aneurism, inguinal, Smith's case of, i. 414.
Anderson's description of the anatomy of the urinary bladder, ii. 55.
Arteria innominata, remarks on a case of ligature of the, i. 215.
Atmospheric air, on the effects of, ii. 267.
Ayre's observations on the nature and treatment of marasmus, and of diseases strictly denominated bilious, reviewed, i. 182.

B.

- Barton's remarks on the virtues of scull-cap**, i. 383.
Baryta and strontia, test for, i. 212.
Bell's history of the contagious fever in Italy, i. 22.
Bilious diseases, review of Ayre's observations on the nature and treatment of, i. 182.
Bilious fever among the blacks in Philadelphia in May 1820, i. 321.
Bladder, description of the anatomy of the, ii. 55.
Blood, difference between fœtal and maternal, i. 8.
Blood, menstrual, fœtal, &c.—Dr. Lavagna's experiments on, ii. 198.
Bony tumours, Gibson on the history and treatment of, ii. 121.
Bond on the effects of atmospheric air, when applied to parts of the body not designed to be in contact with it, ii. 267.

Bond's case of swelled leg occurring in a male, ii. 339.

Bronchocele, Gibson's remarks on, i. 44.

Bronchocele, iodine recommended in, i. 211.

C.

Calhoun on the use of the tourniquet in palsy, i. 131.

Caldwell's case of *tænia*, i. 135.

Calculus in the urethra, Smith's case of, i. 149.

Calorimotor, description of Dr. Hare's, i. 166.

Calhoun on the medical character of the United States, ii. 39.

Carbuncle, Dr. Physick's case of, ii. 172.

Caustic, remarks on the use of, in carbuncle, ii. 172.

Carbonate of iron recommended for the cure of *tic douloureux*, ii. 203.

Calomel and opium recommended in *marasmus*, i. 184.

Chapman on the nourishment of the *fœtus*, i. 1.

Chapman's eulogium on Dr. Dorsey, i. 198.

Chapman on the *modus operandi* of medicines, ii. 295.

Chapman on the pathology and treatment of *cynanche trachealis*, or croup, i. 299.

Cholera morbus of Bengal, history of, ii. 355.

Chincough, Watt's treatise on, reviewed, i. 423.

Cinchonine, a new alkali, discovered in Peruvian bark, ii. 261.

Cicuta, a cure for bronchocele, i. 67.

Combustion, spontaneous, singular instance of, i. 211.

Copper, test for, i. 211.

Corrosive sublimate, gluten an antidote for, i. 209.

Coxe on *materia medica* and pharmacy, i. 34.

Covel's case of uncommon disease, ii. 331.

Cynanche trachealis, on the pathology and treatment of, i. 299.

D.

Deafness, inflation and injection of the Eustachian tube adopted as a remedy for, ii. 200.

Dewees on the rupture of the uterus, i. 73.

Dewees on the retroversion of the uterus, i. 242.

Dewees's strictures on Dr. Merriman's opinions of retroversion of the uterus, &c. ii. 76.

Devèze's treatise on yellow fever, reviewed, ii. 392.

Diabetes, a new hypothesis of the cause of, ii. 201.

Disease, Covell's case of uncommon, ii. 331.

- Dorsey, Dr. Chapman's Eulogium on, i. 198.
 Double canula, Physick on the use of the, i. 17.
 Dudley's case of luxation of the os humeri, i. 417.
 Dysuria, successful treatment of, by Dr. Smith, i. 149.

E.

- Electricity, curious experiment in, i. 211.
 Emetics, effects of, in quartan ague, ii. 334.
 Epistaxis successfully treated by immersion, ii. 329.
 Erysipelas, efficacy of mercurial ointment in, ii. 153.
 Erysipelas, Calhoun's case of, ii, 351.
 Exostosis, history and treatment of, ii. 122, 135.
 Exostosis, Calhoun's cases of, ii. 353.

F.

- False joint united by seton, ii. 337.
 Fever, contagious, in Italy, i. 22.
 Fever, yellow, in Philadelphia in 1820, Jackson's account of, i. 313, ii. 5, 201.
 Fever, yellow, Dr. Devèze's treatise on, ii. 392.
 Fever, bilious, among the blacks in Philadelphia, i. 321.
 Fever, spotted, at Gardiner, Maine, in 1814, i. 167.
 Flora of North America, by Barton, reviewed, i. 420.
 Fœtus, on the nourishment of, i. 1.
 Fomentation, efficacy of, in allaying pain and abating inflammation, ii. 200.

G.

- Galvanism, Hare's theory of, reviewed, i. 152.
 Galvanic experiments of Ritter and Wollaston, i. 153.
 Galvanic apparatus, Hare's memoir on his new modifications of, i. 271.
 Gangrenous suppuration, Dr. Barne's case of, ii. 168.
 Gastrotomy successfully performed, i. 77, 123.
 Gibson on the history and treatment of bony tumours, ii. 121.
 Glover's case of hydrocephalus, ii. 159.
 Gluten recommended as an antidote for corrosive sublimate, i. 209.
 Goitre, remarks on, by Dr. Gibson, i. 44.
 Graduates at the University of Pennsylvania, ii. 205.

- Graduates at the College of Physicians and Surgeons of the University of the State of New-York, ii. 207.
Graduates at the University of Maryland, ii. 208.
Gums, on the devastation of the, ii. 282.

H.

- Hale's history of spotted fever, review of, i. 167.
Hare's theory of galvanism, reviewed, i. 152.
Hare's memoir on galvanic apparatus, i. 271.
Hare's animadversions on the review of his theory of galvanism, i. 371.
Harlan on the generation of animal heat, ii. 250.
Hemorrhoidal tumours, method of extirpating, i. 17.
Horner's observations and experiments on certain parts of the nervous system, i. 285.
Hosack's system of practical nosology, review of ii. 402.
Hydrocephalus, Dr. Glover's case of, ii. 159.
Hydrophobia, Barton's case of, i. 404.

I. and J.

- Jackson's account of the yellow or malignant fever of 1820, i. 313, ii. 5, 201.
Jameson's report on the epidemic cholera morbus of Bengal, reviewed, ii. 355.
Immersion successfully employed in a case of epistaxis, ii. 329.
Intoxication, effects of, in a case of luxation, i. 418.
Iodine, experiments with, as a remedy in bronchocele, i. 211.
Jourdan's historical and critical observations on syphilis, ii. 221.
Jujubes, Barton's account of the large and little, ii. 213.

K.

- Kinine, a new alkali, discovered in Peruvian bark, ii. 261.
Koecker on the devastation of the gums and alveolar processes, ii. 282.

L.

- Lard recommended in the cure of erysipelas, ii. 351.
Lea's remarks on milk-sick, ii. 50.
Lithotomy, description of the anatomy of the bladder and some of its appendages, as concerned in the operation of, ii. 55.

- Little's cases illustrative of the use of mercurial ointment in erysipelas, swelled leg, &c. ii. 153.
 Lumbar abscess, a case of, i. 141.
Lusus naturæ, account of a, by Dr. Livingstone, ii. 143.
 Luxation of the os humeri, Dudley's case of, i. 417.
 Lymphatic vessels, anatomical discovery respecting the, ii. 197.

M.

- Maclurg on reasoning in medicine, i. 217.
 Magendie's experiments on the absorbents, ii. 410.
 Malignant fever of 1820 in Philadelphia, account of, i. 313, ii. 5, 201.
 Mania a potu, Snowden's essay on, i. 191.
 Marasmus, review of Ayre's treatise on, i. 182.
 Marsh miasmata, notice of Dr. Ferguson's paper on, ii. 202.
 Materia medica, and pharmacy, Dr. J. R. Coxe on, i. 34.
 Medical character of the United States, Calhoun on the, ii. 39.
 Medicines, on the *modus operandi* of, ii. 295.
 Meigs on inverted toe nail, ii. 265.
 Medical Society, Philadelphia, officers of the, ii. 203.
 Merriman's opinions of retroversion of the uterus, and extra-uterine conception, examined, ii. 76.
 Mercurial ointment, its efficacy in erysipelas, swelled leg, &c. ii. 153.
 Milk-sick, Lea's account of a disease so termed, ii. 50.
 Mott on ligature of the *arteria innominata*, i. 215.
 Morgan, Dr. John, Rush's account of, i. 439.
 Muscular power, Mease's cases of the gradual loss of, ii. 322.

N.

- Nancrede on the use of Prussic acid in *phthisis pulmonalis*, ii. 66.
 Nervous system, observations and experiments on, i. 285.
 Nitrate of silver, new method of preparation of, i. 210.
 Nosology, Hosack's system of, ii. 402.

P.

- Palsy, on the use of the tourniquet in, i. 131.
 Peale's Galvanic apparatus, i. 284.
 Pharmacy, Dr. Coxe on, i. 34.
 Pharmacopœia of the United States, review of, ii. 367.

- Phthisis pulmonalis, on the use of Prussic acid in, ii. 61.
 Physick on the double canula and wire in the extirpation of
 scirrhus tonsils, and hemorrhoidal tumours, i. 17.
 Physick's case of carbuncle, with remarks on the use of caustic
 in that disease. ii. 172.
 Piperine, a new vegetable alkali, i. 209.
 Poppy, tincture of the, made from the entire plant, ii. 320.
 Premature puberty, case of, ii. 198.
 Prussic acid, Nancrede's observations on, ii. 61.

R.

- Reasoning in medicine, Dr. Maclurg on, i. 217.
 Retroversion of the uterus, Dewees on the, i. 242.
 Rheumatism, remedy for, i. 212.
 Ritter's galvanic experiments, i. 153.
 Rupture of the uterus, Dewees on the, i. 73.
 Rush's account of Dr. John Morgan, i. 439.
 REVIEWS, i. 152, 420, ii. 176, 355.
 Ayre's observations on the nature and treatment of
 marasmus, i. 182.
 Barton's Flora of North America, i. 420.
 Devèze's treatise on yellow fever, ii. 392.
 Hale's history of spotted fever, i. 167.
 Hare's theory of galvanism, i. 152.
 Hosack's system of practical nosology, ii. 402.
 Jameson's report on the cholera morbus of Bengal,
 ii. 355.
 Moore's history of the practice of vaccination, ii. 176.
 Pharmacopœia of the United States, ii. 367.
 Somerville's Thesis on absorption, ii. 408.
 Watt's treatise on chincough, i. 423.

S.

- Scirrhus tonsils, method of extirpating, i. 17.
 Scutellaria lateriflora, or scull-cap, Barton's remarks on, i. 383.
 Smith's case of calculus in the urethra, i. 149.
 Smith's case of dysuria, i. 147.
 Smith's case of inguinal aneurism, i. 415.
 Snowden's inaugural essay on mania a potu, i. 191.
 Somerville's experiments on absorption, ii. 408.

- Spotted fever, Hale's history of, reviewed, i. 167.
 Sulphat of kinine used with effect in quotidian fevers, ii. 264.
 Swelled leg, case of, occurring in a male, ii. 339.
 Swelled leg, efficacy of mercurial ointment in, ii. 153.
 Syphilis, Jourdan's observations on, ii. 221.

T.

- Tænia, a singular case of, i. 135.
 Tænia, remarks on Caldwell's case of, ii. 343.
 Tetanus, results of anatomical inquiries on, i. 213.
 Thyroid gland, case of extirpation of the, ii. 199.
 Thyroid gland, Calhoun's observations on the, ii. 348.
 Tic douloureux, carbonate of iron recommended as a remedy in, ii. 203.
 Tincture of the poppy, a substitute for laudanum, ii. 320.
 Toe-nail, remarks on inverted, by C. Meigs, ii. 265.
 Tourniquet, its use in palsy recommended, i. 131.

U.

- University of Pennsylvania, list of graduates at, ii. 205.
 University of Maryland, list of graduates at, ii. 208.
 University of New-York, list of graduates at, ii. 207.
 Urethra, microscopical observations on, i. 212.
 Urinary bladder, anatomy of the, ii. 55.
 Uterus, Dewees on the rupture of the, i. 73.
 Uterus, Dewees on the retroversion of the, i. 242, ii. 76.

V.

- Vaccination, Moore's history of the practice of, reviewed, ii. 176.
 Vegetable poisons, antidote for, i. 212.
 Veratine, notice of, i. 210.
 Voltaic apparatus, account of several experiments with, i. 153.

W.

- Watt on chincough, review of, i. 423.
 Wetherill's galvanic apparatus, i. 284.
 Wheat flour recommended as an antidote for corrosive sublimate, i. 209.
 Worthington's case of false joint united by seton, ii. 337.

Y.

Yellow fever, Jackson's account of, i. 313, ii. 5, 210.

Yellow fever, Devcze's treatise on, reviewed, ii. 392.

Z.

Ziziphus vulgaris, account of, ii. 313.

Ziziphus lotus, account of, *ibid.*

The present index is designed for both volumes, completing the Journal for the year. In binding the work, the index affixed to the first volume, may be cancelled, as imperfect.



COLUMBIA UNIVERSITY LIBRARIES

This book is due on the date indicated below, or at the expiration of a definite period after the date of borrowing, as provided by the library rules or by special arrangement with the Librarian in charge.

DATE BORROWED	DATE DUE	DATE BORROWED	DATE DUE
C28(842)M50			

COLUMBIA UNIVERSITY LIBRARIES



0062041037



